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ORIGINAL ARTICLES.

THE DIAGNOSTIC SIGNIFICANCE OF THE MITRAL PRESYSTOLIC MURMUR.[1]

BY FRANK DONALDSON, M.D.,

CLINICAL PROFESSOR OF DISEASES OF CHEST AND THROAT, UNIVERSITY OF MARYLAND.

THE characteristics of the typical mitral presystolic murmur are well known and ought to be easily recognized. It is a loud, long sound, rough and vibrating in quality, commonly with a well-marked thrill. It is audible over a fixed and limited position—the mitral area—a circle of about an inch around the point where the apex touches the thoracic wall; the direction of the sound being toward the apex. Its maximal intensity is to the right of the apex beyond where the blood current enters the ventricle. It may be heard indistinctly above the third rib and, very exceptionally, in other directions. Its rhythm is at the end of the long pause immediately preceding and abruptly ending at, and with, the first sound. The rhythm of the presystolic murmur, when the pulsations are slow, is easily recognized by feeling the carotid pulse while auscultating, because the pause before the first sound is much longer than that which follows it. But when the beats are more rapid the distinction cannot be drawn, for the increased pace is gained at the expense of the period of rest, and one pause may be as short as the other (Fagge). The designation presystolic is sufficiently appropriate, since the murmur immediately precedes the ventricular systole. These important facts show that it is found during the diastole of the ventricle. The closure of the sigmoid valves, the filling of the ventricles, and the contraction of the auricle, take place during this period of the heart's revolution. With the murmur we often hear pronounced accentuation of the first sound at the apex, and of the second sound over the pulmonary artery, and, also, when it has existed for some time, a reduplication of the second sound.

The blood in the lesser circulation rushes on through the distribution of the pulmonary arteries into the elastic pulmonary veins, thence directly onward through the auricle into the ventricle, continuing its flow until the ventricle, as well as the auricle, is nearly full, when suddenly the auricle contracts on its contents and completes the distention of the ventricle previous to its systole.

Ludwig and Hess showed that the mechanism of the closure of the left auricular-ventricular orifice does not reside in the valve alone; the surrounding muscles of the ventricle have an active share not merely in floating up the valve curtains, but in reducing the size of the aperture which these valves have to close. The papillary muscles keep the curtains tight during the contraction of the ventricle. The whole action of the heart is screw-like, six or seven distinct layers of muscular fibres crossing one another. The base muscles do their share of the work of closure, the valves promptly complete it. When the muscles of the base are enfeebled, the valve curtains are insufficient to close the orifice (MacAlister).

It is a mistake to suppose that the presystolic murmur is always the same. Usually it is the most prolonged of cardiac bruits. It may be very short; so short, indeed, as to be with difficulty separable from the natural first sound. Occasionally it appears like a tone or accentuation of the first sound. In this case the first sound becomes so sharp and clear that it may easily be mistaken for the second sound, which becomes inaudible at the heart's apex, and, unless careful attention be paid to the rhythm, there is danger that the murmur may be mistaken for the normal sound, instead of an abbreviated presystolic bruit.

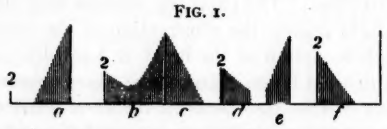
It is a very variable sound both as to its length, its loudness, and its quality. It is heard at some times and not at others. It may, and indeed often does, follow immediately the second sound, and continue up to the culmination of the first sound, with which it suddenly ceases. It is generally more intense at the ending than at the beginning. Traube says that, if the action of the heart be retarded by digitalis, this murmur ends before the first sound. Flint states that only in one of his cases did the murmur cease before reaching the first sound. It often commences in the middle of the diastole with the auricular systole, and runs up to the first sound. Again, it lessens in volume at the middle, and, rising, ascends to its first intensity.

It sometimes becomes altogether inaudible in the middle of the long pause, and then reappears. In some cases there is an entire absence of a murmur, but instead we hear an accentuated clicking first sound.

[1] Read before the Association of American Physicians, September 18, 1888.

The name of post-diastolic is given, very inappropriately, to the murmur heard immediately following the closure of the sigmoid valves. This sound, though rough, is not blubbering and vibrating. It is generally indicative of pronounced stenosis. The diastole of the ventricle begins with the closure of the sigmoid valves. A bruit that immediately follows the second sound, which, it is admitted, is found at this precise period of the cardiac rhythm, ought to be designated diastolic without prefix of "post." These two murmurs, the presystolic and post-diastolic, may be present independently of one another, and when found in the same patient may coalesce, or they may be separated by an appreciable interval.

FIG. 1.



Typical and other varieties of presystolic and post-diastolic murmurs. 1. First sound. 2. Second sound.
a. Presystolic murmur (typical).
b. Presystolic murmur united with post-diastolic.
c. Systolic (mitral insufficiency).
d. Post-diastolic, separated from presystolic.
e. Presystolic. *f*. Post-diastolic.
(After Sansom.)

FIG. 2.

1. First sound. 2. Second sound.
a and *b*. Continuous post-diastolic and presystolic murmurs.
c and *d*. Irregular presystolic.
(After Gairdner.)

Although much has been written of late about the presystolic murmur, yet all who have had much experience in consultations in cardiac cases will confirm the statement of Broadbent, that it is not infrequently, even now, entirely overlooked and often mistaken for the mitral systolic murmur, the differential diagnosis of which has been clearly established. Formerly it was supposed that the presystolic murmur was rarely found, but closer observation has shown that it is of frequent occurrence, especially among women and children. Flint observed it in sixteen out of thirty-nine cases of mitral disease. Its grave prognostic significance alone justifies the attention that has been given to it.

In this paper, I shall only detain the Association by a brief study of the factors entering into the production of this interesting murmur when of organic origin.

There must be two factors in the production of "fluid veins," whether they are obstructive or regurgitant: the lesion, and the propelling force, together with the other well-known influences, modifying blood pressure. For the generation of sonorous fluid veins, we must have the passage of a jet of blood forced through a narrow constriction into a wider cavity or part of a vessel.

It is acknowledged that mitral stenosis is the lesion in a very large majority of cases in which a presystolic murmur is heard. Many prominent authorities believe that this lesion is invariable. The constriction is frequently found at post-mortems in cases in which there had been no presystolic murmur detected during life.

The mitral valves and the auricular-ventricular orifice are specially liable, in early life, to valvulitis from acute or subacute rheumatism and other causes, such as scarlet fever, measles, or chorea. These leave formations of connective tissue which narrow the orifice itself in the annular forms or by diaphragmatic partitions with buttonhole orifices, or the leaflets adhere together and produce funnel-shaped constrictions. In some instances the valves lose completely their natural formation. These new conditions are most favorable to the production of murmurs, which are due to oscillations in the blood itself. The current of blood passes through a small opening into a wider cavity, and is divided up into a number of jets, forming fluid veins. The rapidity and force of the current must cause variations in the intensity of the sounds.

The obstructive lesion of the mitral orifice, it must be borne in mind, is of very gradual formation, of months or of years. It commences often insidiously. The auricular-ventricular orifice is so large that it can bear some lessening of its size without seriously interrupting the proper filling of the left ventricle. Thus stenosis may be sufficient to produce the vibratory sonorous murmur, without any marked subjective symptoms.

While admitting that stenosis of the mitral orifice is, in a very large proportion of cases, the pathological lesion met with where this murmur is found, yet there are numerous cases reported in which the mitral orifices were not altered in size, but in which other obstructive and narrowing conditions were found inside the ventricle—such as shortened chordæ tendineæ, thickenings and rugosities about the leaflets of the valves or in the walls of the ventricle, to account for the production of the characteristic prolonged, blubbering, presystolic murmur. Wherever and whenever we find physical conditions to produce "fluid veins" with vibratory phenomena, we can have the same sound. We need not recall Flint's three cases in which aortic insufficiency, as demonstrated at post-mortems, produced functional murmurs with the characteristics of the mitral presystolic murmur. Dr. Bramwell has published such a case, and Dr. Guitéras reported three. Sansom speaks also of cases somewhat similar to Flint's. Osler has had a well-marked instance.

The writer has never clinically met with such a case, but he has heard aortic diastolic murmurs where there were marked accentuation and shortening of the first sound, similar to the effects met with in cases of mitral stenosis. While it is true that such cases have been reported by the most competent auscultators, they are in fact but rarely met with. There is, however, no *a priori* reason why they could not exist. Aortic regurgitation murmurs are diastolic in time, and if, as Flint claims, the leaflets of the mitral divide the reflow current in the same manner as in the production of the murmur at the auricular-ventricular orifice, the sounds may be formed and a prolonged murmur of presystolic rhythm with the blubbering vibrations may be heard. Dr. Guitéras maintains that this functional presystolic mitral murmur is more frequently heard than Dr. Flint supposed. Balfour, Nixon and Sansom, and others think differently. There can be no doubt that the diastolic aortic murmur can be conducted to the apex by the solid walls of the hypertrophied ventricle, the current of blood itself as well as by the sternum and walls of the chest. The infrequency of these functional mitral murmurs, when there is an aortic insufficiency, is difficult to explain.

Dr. Balthazar Foster suggests an explanation of how this murmur is heard best at the apex: That different valves are affected in different cases, and that, if the left sigmoid near the mitral is affected, the blood falls directly back into the ventricle and the bruit is heard best at apex; whereas, if the right or posterior sigmoid valve is diseased, the regurgitant blood impinges on the septima and the bruit is carried down the right side of the heart. Dr. Guitéras maintains that these propagated murmurs are in fact mitral obstructive murmurs, and that they are more apt to develop when the posterior aortic segment is affected, because in such cases the recurrent stream is brought directly against the anterior leaflet of the mitral valve.

Dr. Bramwell (*Diseases of the Heart*) refers to a case of rupture of the posterior coronary segment of the aortic valves in which no presystolic murmur was audible. There must be some unusual physical condition which has not yet been appreciated.

There is also a murmur which Barlow, Leaming, Flint, and Donaldson have designated as intraventricular, which, when there is much hypertrophy of the left ventricle, seems to occupy the first portion of the systole. This resembles the qualities of the presystolic, and, when the heart is beating rapidly, may be confounded with it. Of course, in diagnosticating mitral stenosis, everything is taken into consideration, together with the murmur.

Let us inquire from what sources the necessary force of the blood-current is derived. Dr. Gairdner taught that it is produced by the systole of the left auricle, and he accordingly named the adventitious sound the "auriculo-ventricular murmur." This could not possibly apply to the sound which exists before the contraction of the auricle, which, in health, occupies one-fifth of a second of time, and immediately precedes the systole of the ventricle. The feeble force of the auricle in health was shown by Chauveau, who, by prolonged irritation, exhausted its contractility, and found that the ventricle continued to act effectively, and kept up the circulation. Ludwig confirmed this result by introducing a tube through the auricle. In fact, the auricle may be regarded as the dilated termination of the pulmonary veins, acting as a reservoir to supply the ventricle, and to regulate the blood-pressure in it. The auricular cavities are never completely obliterated during contraction, though this does not appear to be the case with the auricular appendages.

We may ascribe the production of the presystolic murmur, in beginning obstruction of the mitral orifice, to the force of the left auricle. Temporary compensation is promptly established in the force of the circulation. It seems that there is a reserved force, as in involuntary muscles, which is called forth in the emergency.

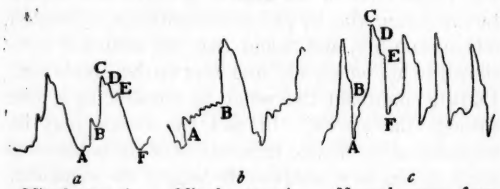
We further admit that, as the result of obstruction at the mitral orifice, impeding the influx of blood into the left ventricle during the diastole, the left auricle, having extra work to perform, does become first dilated and subsequently hypertrophied. Indeed, percussion, in rare instances, reveals abnormal bulging over the auricle, and frequently the pulsation of the auricle is perceptible about the fourth rib. The walls of the left auricle, which are normally about one and a half lines in thickness, may be increased to as much as a quarter of an inch, with proportionate augmentation in strength. It may be that the prolongation of this bruit is due to this hypertrophy, but it is almost incredible when we know that it is the longest, as well as the most irregular and vibrating, of all cardiac murmurs. We question whether it is a force sufficient by itself to overcome the resistance caused by advanced mitral constriction, and to give us the acoustic characteristics of the presystolic murmur.

It must be borne in mind that the auricular systole does not occur until the very last portion of the diastole, and the murmur often begins after the second sound, and includes the so-called "post-diastolic." These phenomena occur previous to the auricular systole. We readily admit that its influence is shown in reinforcing the intensity of these murmurs.

In mitral stenosis the auricular portion of the cardiagrams is of longer duration, and its tracing higher, because the left auricle is dilated, and contracts with force, emptying itself with difficulty; it often shows a series of wavelets, giving it a serrated appearance corresponding to the purring thrill felt

by the hand. Ott, Hess, and Galabin attribute the late closure of the pulmonary valves to the diminished elasticity of the artery in consequence of long-continued high pressure.

FIG. 3.



a. Mitral stenosis. Pulse rate 80 (after Galabin).

b. Mitral stenosis. Long, rough, presystolic murmur. Pulse rate 57 (after Galabin).

c. Normal curve from man (after Landois).

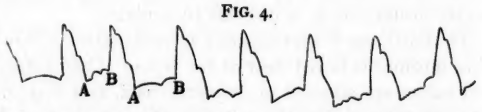
A B. Auricular systole. D. Aortic closure.
B C. Ventricular systole. E. Pulmonary closure.
E F. Ventricular diastole.

The cardiagram of the normal heart (see Landois's *Physiology*) proves that the aortic valves close a little before the pulmonic valves. They close, however, so near together that only a single sound is heard. In mitral stenosis (stage of non-compensation) the aortic valve closes sooner, and the pulmonic valves later than normal. This is because the left ventricle contains less blood, and the right more than normally. There results what is called a reduplicated second sound, which is an increase of a physiological condition. All conditions which cause the aortic valves to close rapidly (diminished amount of blood in the left ventricle), and the pulmonary valves to close later (congestion of the right ventricle)—both of which exist in mitral stenosis, favor the production of a reduplicated second sound, which occurs in one-third of the cases of mitral stenosis.

If mitral contraction occurs at a very early period of life, whether as a primary affection, or secondary to a corresponding narrowing of the tricuspid, other parts of the circulatory system become accommodated to it, and the patient may reach adult life before the lesion produces any symptoms—the post-mortem revealing the fact of the organic defect. Dr. Wilkes reports cases in which resulting hypertrophy of the right side in children was so great as to cause bulging of the chest, and in which the pulmonary artery partook of the hypertrophy, and became thicker than the aorta. On the other hand, cases occur in which, owing to mitral stenosis early in life, so small a quantity of blood is thrown into the left heart that there is imperfect nutrition of the whole body, which accommodates itself to the defect. Hypertrophy of the right ventricle and the left auricle compensates so thoroughly for the deficiency, that there may be no subjective symptoms, and no bruit, perhaps, beyond an imperfect first sound.

Dr. Gairdner did not ascribe the production of the presystolic murmur wholly to the muscular contraction of the auricle. Dr. Wilkes held that the murmur might anticipate the auricular systole, and that it might occur not only at that time, but also during the heart's diastole and pause. Dr. Galabin came to a like conclusion from the evidence afforded by the cardiograph. Dr. Sansom claims that the causation of the presystolic murmur may be independent of the auricle; he states "that in many cases he had observed that though there had been a prolonged presystolic murmur commencing in the long pause almost immediately after the second sound, cardiographic evidence has shown the auricular systole to occupy its normal position just anterior to the commencing contraction of the ventricle." He cites a case in which a murmur occupied at one time a portion and at another almost the whole, of the long pause, and the autopsy showed that the auricular systole could have had no share in producing such a murmur, for not only was the left auricle so dilated that its walls could have exerted no appreciable muscular power, but it was lined by a closely adherent, old, laminated blood-clot. It thus appears

FIG. 4.



Sansom's case, showing mitral stenosis, while auricular systole is in normal position. A B. Auricular systole.

that the post-diastolic murmur, as well as the presystolic, may be caused in the diastole, and be due to the entrance of the blood independently of the auricular systole, it being urged through the stenosed aperture, owing to the tension under which it has been retained in the elastic and distended auricle and pulmonary veins.

We feel justified in concluding that the systole of the left auricle is a factor of greater or less force in the production of this mitral direct murmur, but it is not the only one, and the others may produce it independently of the auricular systole.

Negative pressure. Let us see whether any negative pressure is brought to bear upon the left auricle and pulmonary veins to assist in getting the current into the left ventricle. Is there any suction action of the left ventricle of the heart? Until the publication, in 1878, of Goltz and Gaule's experiments, the majority of physiologists attached little or no importance to any suction action of the heart, although in all times there have been zealous advocates of the doctrine that diastolic suction was an important factor in the circulation. Goltz and Gaule, by the use of ingeniously constructed maximum and minimum manometers, determined the maximum and minimum pressures in the left and in the right ventricle. They found with natural respi-

ration, in the left ventricle of a dog, a negative pressure of 52 mm. of mercury. In the right ventricle there existed, in another case, a negative pressure of 17.2 mm. of mercury.

In accordance with the generally accepted ideas, they at first were inclined to attribute this negative pressure to the expansion of the chest during inspiration. To their great astonishment, however, they found, after the chest was opened, and independently of respiration, a negative pressure in the left ventricle of 23.5 mm. of mercury. This latter negative pressure, therefore, cannot be due to the elastic traction of the lungs. They attribute it to the elastic resilience of the ventricle following the systole, like that after compression of an elastic balloon. They say: "The adherents of the doctrine, according to which the heart acts as a suction-pump, will find their boldest expectations surpassed when they hear that the suction force of the left heart of a large dog, at the beginning of the diastole, is equal to a column of water 320 mm. (23.5 mm. of mercury), and that this force in a healthy man is probably even much higher." (Goltz und Gaule: "Ueber die Druckverhaltnisse im Innern des Herzens," *Pflüger's Archiv*, 1878, Bd. 17, S. 100.)

In 1879, Moens published an article ("Die erste Wellengipfel in dem absteigenden Schenkelder Pulscurve," *Pflüger's Archiv*, Bd. 20, S. 517, 1879), in which he attempted to show that the negative pressure found by Goltz and Gaule existed, not during the diastole, but during the systole. In order to make clear how a negative pressure might be produced at this period of the heart's action, he takes an elastic tube open at one end and connected at the other by means of a stopcock with a pressure-bottle. If the stopcock be suddenly closed while the fluid is flowing from the bottle through the tube, there is a negative pressure produced in the tube, beginning near the stopcock. Likewise, in the left ventricle, he argues that, as the blood is shot out by the ventricular contraction into the aorta, a negative pressure is produced in the ventricle. His chief inference, that the negative pressure is present during the systole and not during the diastole, was because a minimum manometer, passed down the jugular vein into the superior vena cava, failed to register negative pressure, as would be the case if the right ventricle exerted diastolic suction. He regards this experiment as proving that the negative pressure is not during the diastole, otherwise there would be a suction force exerted on the blood in the right auricle and vena cava superior. If the negative pressure is not during the diastole, it must be during the systole.

It will be observed that Moens does not actually demonstrate that the negative pressure is during systole. He infers it indirectly. On the other hand, he seems to have overlooked the fact that Goltz and Gaule found a negative pressure in the right auricle of 11.2 mm. mercury (this might have been due to inspiration), the negative pressure in the right ventricle, in the same case, being 17.2 mm. mercury. Moens is quoted by Martin and Donaldson, Jr., as having shown that the negative pressure exerts no suction force on the blood in the auricles.

In consequence of the doubt which Moens's work raised as to the period at which negative pressure existed in the ventricles, de Jager, in 1883 ("Ueber die Saugkraft des Herzens," *Pflüger's Archiv*, Bd. 30, S. 491), published experiments on this question, and came to conclusions in opposition to Moens's and in confirmation of Goltz and Gaule's. De Jager determined that the pressure at the beginning of the aorta never becomes negative, as it should be in case the negative pressure in the ventricle were due to the suction of the column of blood poured out from the ventricle. He also found that with the chest open, so that the action of inspiration on the aspiration of blood into the right heart was removed, the pressure in the right auricle becomes negative. He found negative pressures in the right auricle of 2 mm. to 6 mm. mercury. He does not believe that the auricles themselves are capable of exerting any suction force, and he, therefore, concludes that this negative pressure in the right auricle can be interpreted only as evidence of a suction exerted by it, which, of course, can occur only during diastole. The same conditions would, of course, obtain for the left cavities of the heart, as the experiments were made with the chest open. De Jager, therefore, concludes that a strong diastolic suction force is exerted by the ventricles.

As regards the causes of this suction force he mentions several possibilities, without coming to a decision. These possibilities are:

1. The elastic force developed during the systole, as in the compression of a rubber balloon.

2. The suction force on the exterior of the heart resulting from the negative pressure in the thorax (or the elastic traction of the lungs). This, of course, can be only one element, as negative intraventricular pressure is found after the thorax is opened.

3. Shortening of the large arteries coming from the heart.

Brucke inclines, as do Goltz and Gaule, to consider these factors as the essential ones.

Gruenhagen holds that Goltz and Gaule and de Jager have established the fact of a diastolic suction action of the heart.

The experiments of Martin and Donaldson, Jr., however, on the mammalian heart, at the Johns Hopkins University, are opposed to the existence of any suction action of the heart. Their experiments are exact, and, it would seem, quite conclusive

against the suction theory. It is difficult, however, at present, to reconcile them with Goltz's, Gaule's, and de Jager's experiments. They have the merit of putting the matter to a direct test, whereas the other experiments do not actually demonstrate a suction action, but leave it as an inference. They say:

"The negative pressure proved by Goltz and Gaule to occur in the ventricles for a brief period at the end of the systole, had already been shown by Moens not to affect the auricles, and, therefore, to be without effect in making the heart a suction pump as far as the venous system was concerned.

"Once the 'aspiration of the thorax' has been eliminated, the right auricle of the mammalian heart will not receive blood unless supplied to it under a decided, if small, positive pressure. While the heart in the closed thoracic cavity may, and probably does, act as a suction pump, this is not due directly to an active expanding force of the heart, but is the secondary result of the pneumatic conditions prevailing within the normal closed chest cavity."

If a suction action exists, it is evidently a factor not to be neglected in the study of mitral stenosis. All that increases the suction action, increases the force with which the blood passes through the narrowed mitral orifice and doubtless increases the murmur. De Jager found that when the action of the heart became feeble—as after long exposure, or by cooling, or by loss of blood—then only a slight negative pressure, or none, was produced in the ventricles. We may, therefore, infer that all circumstances which enfeeble the action of the left ventricle diminish correspondingly the force of suction, and, therefore, the murmur. Such circumstances are failure of the heart's force from any cause, among which may be noted atrophy of the left ventricle in pure mitral stenosis. In general, whether the left ventricle is atrophied or not, the small amount of blood which it receives through the narrowed mitral orifice during non-compensation must be attended with a weaker systole, and, consequently, diminished suction and lessened murmur. All this line of argumentation, of course, is not pertinent, if we accept the doctrine that no suction force is exerted by the ventricles.

Let us further inquire whether any other force is connected with the passage of the blood through the left ventricle. We hear of the blood's passive flow through the mitral orifice. Has it no power? The right ventricle, which propels its contents with a strong wave through the pulmonary arteries and with comparative ease, owing to the absence of tonus or resistance, and of vasomotor influence to contend against, forces the blood into the pulmonary veins and through the left auricle into the ventricle. If it meets with any obstruction, the backward pressure would, of itself, add to the onward blood pressure in the pulmonary veins, although the right ventricle promptly exerts its reserved compensatory force. If the obstruction continues, the extra work of the right ventricle gradually produces permanent hypertrophy which keeps up the compensation. When this is broken, there results the disastrous backward flow of venous blood with all its unpleasant consequences upon the lungs, liver, and other internal organs. This occurs, notwithstanding the protection afforded to the process of intra-pulmonary respiration, as shown in the safety-valve construction of the tricuspid valve (King), of the pulmonary semilunar valves (Flint, Jr.), and of the obstruction safety-valve principle of the pulmonary valve (Guitéras). The increased tension at the pulmonary valves gives accentuation of the second sound. Nature further provides a relief to the lungs by the increased capacity of the right cavities by from one-tenth to one-third over the corresponding cavities of the left side.

The right ventricle becomes very gradually hypertrophied. In some cases the enormously thickened walls completely cover the left ventricle and prevent our hearing the second sound at the apex and a very feeble impulse is noticed. The left ventricle, owing to the insufficient supply of blood through the auricular-ventricular orifice, is often not only not hypertrophied, but actually materially lessened in size—indeed, in a state of atrophy, unless there is also insufficiency of the mitral orifice as a complication. This additional lesion may cause hypertrophy of the left ventricle. May not the powerful right ventricle, by the blood pressure it causes in the pulmonary veins, be a prominent factor in causing variations in the length and intensity of the post-diastolic and the presystolic murmurs in mitral stenosis?

If this hypertrophy be very marked, may it not be the propelling force producing the long sound commencing immediately after the systole and occupying the whole of the diastole? If the force be feeble, it may only produce the post-diastolic and cause the deficiency in the middle of the distance between the second and first sound. When the patient is in the recumbent position and the heart is quiet the murmur is often inaudible; on changing the position it may be heard. Active exercise increases in marked degree the intensity of the murmur. When dilatation succeeds, hypertrophy of right ventricle and tricuspid insufficiency take place—the forces are not powerful enough to cause the murmur. The auricular systole, if dilatation has not made the walls too feeble, may give rise to the presystolic. The increased power of the hypertrophied left auricle and right ventricle certainly would cause the fluid veins to be audible vibrations in obstructive mitral disease. As the muscular power leaves the excessively enfeebled patient, the sound utterly disappears. Indeed, it is very remarkable how the murmur sometimes dis-

appears, temporarily or permanently, the condition of valve being unaltered.

To sum up (says Dr. Galabin, in his elaborate and able paper on, "Interpretation of Cardiographic Tracings," *Guy's Hosp. Reports*) the inferences suggested by a general review of all the tracings, it appears that the evidence of the cardiograph is in favor of the view that two totally distinct murmurs may be caused by mitral contractions: first, the auriculo-systolic bruit, which may either run up to the first sound or be separated from it by a short interval; and, secondly, a diastolic bruit, due to the venous flow through the narrow and roughened orifice, which in rare cases may be blowing in quality and separated from the succeeding systole by a long pause; and that, thirdly, these two may be merged together into a compound murmur somewhat rough from its commencement, but much intensified in loudness and harshness toward its conclusion.

Marey, in some experiments by means of which he reproduced the various sounds and murmurs of the heart, the auricle of which was not contractile, introduced a plug perforated by a hole between the auricle and ventricle, and then found that a diastolic murmur was produced, but only when the auricular pressure exceeded a certain point. Hence, he concludes that the murmur of mitral stenosis may be either diastolic or auriculo-systolic according to circumstances.

How far is our estimate of the pressure in the pulmonary veins justified by experimental research? From this source we possess but little positive knowledge. Beutner seems to be the only authority who has published any actual measurement of the blood pressure in the pulmonary veins. He found a positive pressure of 10 mm. of mercury in the pulmonary vein of a cat. In the pulmonary artery of a cat he found a pressure of 17.6 mm. mercury. These pressures were, of course, measured with the thorax open, so that we cannot attach absolute value to them. They indicate however, that, relatively to the pressure in the pulmonary artery, the pressure in the pulmonary vein is high, the pressure in the veins being more than one-half that in the artery. The pressure in the pulmonary vein, according to Beutner's measurement, is, moreover, absolutely a high one when compared with the very low pressure in the veins emptying into the right auricle. The pressure which Beutner found in the pulmonary artery is much lower than has been found by subsequent observers. The accepted belief is that the pressure in the pulmonary artery is one-third of that in the aorta; Goltz and Gaule regard it as two-fifths of the aortic pressure. If the ratio which Beutner found between the pressure in the pulmonary vein and that in the pulmonary artery holds true, then we may conclude that the pressure in the pulmonary vein is even higher than his observations indicate.

Inasmuch as actual measurement of the pressure in the pulmonary vein can be made only when the thorax on one side is open, the question arises: What influence upon this pressure has the elastic resilience of the lungs under the normal conditions of respiration? If the walls of the pulmonary artery and those of the pulmonary veins were of the same thickness, then, inasmuch as both vessels were subjected to the same intra-thoracic pressure, it is evident that changes in the intra-thoracic pressure, corresponding to the different phases of respiration, would not influence the blood pressure. The walls of the pulmonary veins, however, are thinner than those of the pulmonary artery, therefore the aspiration of the thorax must tend to lower the pressure in the pulmonary veins more than in the pulmonary arteries. It is impossible to say how great is this influence of the elasticity of the lungs upon the pressure in the pulmonary veins.

The feeble tonus of the vessels in the pulmonary circulation is of the utmost importance, as changes in pressure in the pulmonary veins are quickly manifested in the pulmonary arteries and *vice versa*. This circumstance justifies us in assuming that change of pressure in the pulmonary artery is attended by corresponding changes of pressure in the pulmonary veins, so that changes in the force of contraction of the right heart exert a much more direct influence upon the circulation of blood in the pulmonary veins than in the left heart and the systemic veins.

Cohnheim says: "The blood in the pulmonary veins is, under physiological conditions, not like that of the systemic veins under negative pressure when it enters the heart, but is under a positive pressure which is only a little less than that of the blood in the pulmonary artery." (*Vorlesungen, über allgemeine Pathologie*, Bd. 1, S. 28, Berlin, 1882.) This statement of Cohnheim does not seem to rest upon actual measurement, but to be based upon the slight resistance offered to the blood in the passage from the pulmonary artery to the pulmonary vein, and upon Beutner's measurement with open thorax. He found the pressure in the pulmonary vein more than half that in the pulmonary artery. As no means have been found which enable us to measure the pressure in the pulmonary veins under natural conditions, we are left to such inferences as the above.

PERSONAL OBSERVATIONS ON SKIN DISEASES IN THE NEGRO.[1]

BY ROBERT B. MORISON, M.D.,
OF BALTIMORE.

THE study of skin diseases in the negro offers some points of interest which it may perhaps be well to

[1] Read before the American Dermatological Association at the Congress of American Physicians and Surgeons in Washington, 1888.

note in order that the attention of those among us who come into contact with this race may be called more particularly to the difference of the lesions as compared to the white race.

A difference in the symptoms and general appearances would, from the nature of the race, suggest itself before experience had taught one the fact. In a dispensary such as the one here quoted from, where fifty per cent. of the skin cases are black or modifications of black, an opportunity is offered for close observation of certain peculiarities which deviate from the ordinary or orthodox symptoms seen in the whites. It may be due to this race that the American dermatologist is so often quoted as saying there is a difference between skin diseases in America and in Europe. The negro race does present a modification of such diseases, thereby rendering the total average of skin symptoms somewhat different to the European stereotyped forms, but when considering the white race the specialist, from my own observation, meets the same diseases here as there, leaving out of the question those which are rarely, if ever, seen in this country.

There is, however, something in the appearance of skin diseases in the black race which might confuse an European specialist who was unaccustomed to seeing them, for they seem to be less liable to skin diseases of a severe type than the whites. They suffer in the aggregrate as often, but far less severely. Their skins are less sensitive to heat, poisons, irritants, nervous lesions—disease of the nerves being a theoretical method of explaining unknown causes—to constitutional or to general influences.

My attention being attracted by such differences, notes were begun by which a more careful comparison could be made over a series of years and a certain number of cases. In the record made on the dispensary books the color of the patient was noted, whether dark or light—that is, whether true black or mulatto, and they have thus been kept for three years, making a sufficient number to show by percentage and histories wherein the differences have presented themselves to me.

It is hardly necessary to state that the normal skin of the black race is histologically more coarse and thick than that of the white. It is composed of more dense fibrous tissue, the glands are larger as are also the nerves and vessels running into the papillary layer. The epidermis is thicker and slightly yellowish in color, thereby adding somewhat to the general color of the skin. The blackness of the pigment granules determines the color of the skin, there being no appreciable difference in the thickness of the pigment layer between the true black and the mulatto. There are fewer elastic fibres in the negro skin and more subcutaneous adipose tissue than in the white. The nerves, though larger, are less sensitive both to external and internal agencies. These histological differences may help to explain the modified symptoms which their skin diseases present.

I have tabulated, for the purposes of comparison, 500 cases of each race—the black and the white—and shall run over each disease as it comes up alphabetically. These cases were taken from the last three years of the dermatological clinic of the Baltimore Polyclinic and represent all the cases without exception up to 500 which have been recorded within that time.

The table of the blacks is divided into those who are black or true negroes and those who are mulattoes. I consider this an important division, as most generally the nearer the color approaches to white the less difference there is in the lesions.

The first disease on the list is *Acne*. Acne in the black is a rare disease. Out of my 500 cases only 9 are acne and they all were mulattoes. The white list shows 39, which is four times as many. Out of all the cases of the negro seen in private or public practice and of all the negroes purposely noticed in passing, it has been rare to discover an acne in the black. I have had only one case in private practice.

TABLE OF 500 CASES OF WHITES.

	Male.	Female.	Total.
Acne	22	17	39
Alopecia areata	...	1	1
Angioma	...	1	1
Chancre	4	1	5
Chilblain	3	...	3
Chloasma	...	2	2
Dermatitis ven.	2	4	6
Eczema (acute)	68	92	160
" (chronic)	12	13	25
Elephantiasis Arabum	...	1	1
Epithelioma	2	1	3
Erysipelas	...	5	5
Erythema multif.	4	3	7
" nodosum	...	1	1
" papulat.	...	2	2
" urticans	...	2	2
Favus	1	1	2
Furunculosis	3	4	7
Herpes	...	4	4
" zoster	13	4	17
Ichthyosis	1	1	2
Impetigo contag.	3	9	12
Lichen planus	3	1	4
Pediculi	4	1	5
Prurigo	3	...	3
Pruritus	2	3	5
Psoriasis	11	15	26
Purpura hem.	1	1	2
Scabies	5	6	11
Sycosis non par.	2	...	2
Syphilis, secondary	21	19	40
" tertiary	13	18	31
" congenital	4	4	8
Tinea tonsurans	8	6	14
" versicolor	5	3	8
Tylosis	...	2	2
Ulcer (syphilitic)	7	8	15
" (varicose)	...	3	3
Urticaria	4	7	11
Varicella	7	1	8
Verruca	1	...	1
Vitiligo	...	1	1

TABLE OF 500 CASES OF NEGROES.

	Black.		Mulatto.		Total.
	Male.	Female	Male.	Female	
Acne	...	...	2	7	9
Ainhum	2	...	...	...	2
Chancre	...	...	1	1	2
Chilblain	2	3	3	2	10
Chloasma	...	3	...	...	3
Elephantiasis Arabum	...	2	...	...	2
Erythema multif.	...	2	...	...	2
" nodosum	...	...	...	2	2
Eczema (acute)	18	22	24	32	96
" (chronic)	6	20	3	4	33
Favus	2	...	...	...	2
Furunculosis	...	...	...	2	2
Herpes	...	...	4	6	10
" zoster	1	1	...	2	4
Impetigo contag.	2	2	1	4	9
Keloid	...	3	..	...	3
Lichen scrofulosus	1	...	...	...	1
Lupus vulgaris	...	2	...	...	2
Lymphadenitis	...	...	7	8	15
Measles	...	1	...	...	1
Molluscum contag.	1	...	...	...	1
Pediculi	6	4	2	...	12
Pruritus	9	15	...	...	24
Psoriasis	2	...	...	4	6
Scabies	2	3	...	1	6
Seborrhœa cap.	...	2	2	8	12
Sudamina	1	...	...	...	1
Sycosis non par.	1	...	...	...	1
Syphilis, congenital	...	...	5	...	5
" secondary	20	20	35	16	91
" tertiary	12	5	3	2	22
Tinea tonsurans	15	16	6	5	42
" versicolor	...	...	...	2	2
Ulcer (syphilitic)	3	4	4	1	12
" (varicose	1	3	...	...	4
Urticaria	1	2	6	8	17
Varicella	2	...	8	12	22
Verruca	3	5	...	...	8
Vitiligo	2	2	...	...	4

It appears in a mild punctate form, does not become indurate, and is easily cured by local applications. Itching accompanies it, according to the statement of the patients. No cases of rosacea, of rhinophyma, or coachman's nose, were seen.

The diagnosis is easy, differing from the punctate form of the whites only in the color of the surrounding skin and in the absence of redness in the papules.

Ainhum is a disease of the negro race and the two cases reported were both in black negroes who were sailors. One of them had previously lost the little toe of the left foot, he being now troubled with the disease in the little toe of the right foot. There was no history of self-mutilation. They seemed both above the average negro in intellect.

Chancre presents no differences excepting in the amount of induration, which is greater in the negro. One of those reported here and one of those reported in the white list were upon the upper lip of a female.

Chilblain is a common disease in the negro. It is less intense in the inflammation than in the white, but there is more itching and it is more chronic. In this record of ten cases there is no difference between the frequency of it in the black and in mulattoes. It seldom is so troublesome as to call for a physician's advice, but there are few negroes who will not say when asked, as I have found, that they are martyrs to chilblain.

Chloasma is a common disease in negro women. Few think it necessary to undergo treatment for it, but I have often noticed it when they come for some other trouble. Its appearance is that of a loss of pigment. In the black woman there is a yellowish spot on a dark back ground. Treatment is consequently reversed: we must produce pigment not abstract it.

Elephantiasis Arabum in the negro in this country usually follows syphilis, as in the two cases here observed. One of these was diseased in both vulvæ, which were finally removed by the knife. An interesting arrangement of the pigment in the histological specimens made from this case was reported previously by the author in THE MEDICAL NEWS of April 23, 1887.

Erythema multiforme appears in small or large spots without redness, but with burning and itching which so much overshadow the other symptoms that doubtless many cases are overlooked. There has been such violent scratching as to produce a confusion in diagnosis. The cases here reported were in their incipiency, on the back of the hands and arms of two black females who washed for a living. The patches were slightly indurated raised above the surrounding skin, different in size, whitish upon their surface, and rendered pale upon pressure. In places there was evidence of scratching and rubbing, so that some of the patches were obscured.

Erythema nodosum is represented by two mulatto women and was situated upon the legs below the knees. The nodes were raised, copper-reddish, large, indurated, painful, and itching. They disappeared promptly under tonic treatment. The pain in these cases was less than in the whites, but the itching greater.

Eczema. Acute eczema is much less common and much less severe in the negro than in the white race. Out of 500 cases there were recorded 96, which is about 19 per cent. This is a much smaller percentage than is usual, for we see out of the 500 whites there are 160 acute eczemas, which is about 30 per cent. of the whole, the usual percentage in dispensaries. Looking at the record more closely, we notice that there are fewer cases among the true negro than the mulatto—40, or less than half, being dark. The majority of cases reported among the dark females are due to washing and are localized upon the hands.

I have no case of acute universal eczema recorded among the dark male or female. There are favorite situations for it to appear in this class, such as the instep or heel, the back of the hand, the neck, the knee-joint, and armpit. It appears in spots, dry and scaly, with a grayish-white, dirty-looking, thin crust covering them. They differ in size, from a ten-cent

piece to twice the size of one's hand. They itch constantly, but not intensely. They have the appearance of spots of dust upon a black coat, which may be brushed off. E. rubrum, papulosum, or vesiculosum universale I have not seen among them. The babies, however, do sometimes have a modified form of milk-crust, which approaches nearly to an ordinary moist eczema.

Among the mulattoes there is little difference between them and the whites in their symptoms. Their babies suffer from milk-crust just as frequently, and they themselves pass through an attack of acute eczema in the same way. They seem, however, more amenable to treatment, and less liable to intense itching than the whites.

In chronic eczema we find that the black negro outnumbers the mulatto. Of the 20 black females, 15 have washerwoman's eczema of the hands, the other 5 being spots upon the head or neck. The men have had theirs produced by oyster shucking or other occupations. These eczemas take on a form of keratosis, which nothing but change of occupation will cure.

Chronic eczema in the mulatto does not differ from that of the whites, other than that the itching is less acute.

Favus is rare in the negro. The short wool which covers the head, and which, even in babies, is, according to their custom, most often kept closely shorn, accounts for this fact. The parasite does not get a chance to grow. It is easily got rid of by treatment.

Furunculosis is a disease which is apparently infrequent in the negro, although the smallness of the number in this record is probably due to the fact that such cases are turned over to the surgeon.

Herpes facialis is a common disease among negro babies; so common that it is rarely thought necessary to consult a physician. It is, to a great extent, produced by the so-called "sugar-teat" which pickaninnies are allowed to suck constantly. The "sugar-teat" is a piece of fat bacon dipped into brown sugar or molasses, and wrapped in a piece of old linen cloth. This form of herpes takes the place of milk-crust in the negro, and is the only form of simple herpes here recorded.

In *herpes zoster* the pain preceding the eruption in the black is less severe, and less prolonged than in the white. There is decided itching rather than pain or burning, and when the vesicles appear they are white, on a black, somewhat indurated ground, containing the usual quality and quantity of serum. They sometimes leave—as in one of the cases here reported—pits, with loss of pigment, which last the rest of life. Slight keloidal growths sometimes follow in the wake of the largest vesicles.

The symptoms of *impetigo contagiosa* are indistinct, and not so characteristic as in the whites. The eruption is not profuse; there is little or no coalescing of pustules. There is considerably more itching, and the crusts formed after scratching are more yellow. Upon their removal they disclose a slightly reddish, glossy surface, which soon heals, leaving for a while a whitish spot. It is not very contagious, and it may be confounded with herpes in diagnosis.

Keloid. Negroes are much more prone to keloid than the whites. Two of the cases here reported were the result of smallpox, the keloidal growths surrounding the pits, making a network of hypertrophied eschars resembling the surface of a raised map. Such a result is not uncommon after smallpox in the negro. The third case reported followed piercing of the ears for ear-rings, and the size of the growth was gradually increasing. All these cases were in the black or true negro. Keloid in the mulatto is almost as rare as in the white race, and it does not grow with such rapidity as in the black.

Lupus appears to be uncommon in the negro. In view of the fact that this disease is now called tuberculous, it seems rather incongruous. The negro is supposed to be especially susceptible to tuberculosis. He lives in a meaner and far less healthy hygienic condition than his white neighbor; the moral tone and promiscuous intermingling of the sexes are not conducive to strength of body or cleanliness of the skin, and yet a disease like lupus, which is apparently the outcome of such poor surroundings, is rarely seen among them. One of the cases recorded was that of a black girl, nineteen years old, whose cheeks, nose, chin, and lips were diseased. The mucous membrane of the nose and pharynx was also affected. Spooning and cauterization resulted, as might have been foretold, in the formation of a keloid half an inch wide, occupying the outer edge of disease upon the cheeks. Progress in this direction was stopped, while the rest of the surface within the keloidal circle has from time to time broken out afresh.

By the application of mercurial plasters and the use of electrolysis the keloid has been diminished to one-third its former size. There is a curious-looking white line an eighth of an inch in width stretching along the outer border of the keloid and the tubercles of the diseased portion heal by leaving a whitish spot behind. The patient is a large, robust, otherwise fine-looking young woman without any symptoms of tuberculosis elsewhere.

Lymphadenitis. Universal enlargement of the glands is very common in the mulatto, not so much so in the black. It is more common in both than in the white race. The lymphatic glands and vessels are histologically larger and are easily affected by an inflammation. Being liable to glandular enlargement from slight causes other than syphilitic such enlargement does not mate-

rially assist in making a diagnosis of specific or of syphilitic disease. The cervical, inguinal, and other superficial lymphatic glands of young healthy negro children are often quite prominent and remain so during life. Suppuration is slow, while absorption of an enlarged gland by any means tried is seldom successful.

In the case of *molluscum contagiosum* observed the disease was upon the scrotum and penis, resembling the same disease in the white excepting that the contents of the small tumors were darker in color and mixed with pigment granules. Whitish spots remained for a while after the removal and healing of the tumors.

Pediculi. For the same reason that favus is rare—that is, because the head is so closely shorn, do we seldom see head-lice in negro children or men. The lesions produced by body-lice are slight and oftentimes unnoticed by the one bitten. The black man is somewhat impervious to the louse bite as he is to bed-bugs and mosquitoes. The poison of these venomous insects seems to be lost on the ebony color and texture of their skins.

Pruritus. Under this head I place all the troubles of the skin in the negro in which there is itching but no visible eruption. Apparently they have some of the diseases which white people have only in this form. It is very common to hear them complain of what they call an "eetching in the blood." When uncovered there is nothing to be seen but a rough black skin which feels hard and dry. The surface has lost its gloss and its elasticity is lessened. This condition of the skin I have only seen in the black, not in the mulatto. It is most often localized in such places as the front of the arms and legs and sides of the abdomen. The itching is constant, but not severe enough to cause traumatic scratching. The troublesome places are rubbed with the hand, seldom scratched. It does not fly from spot to spot, does not interfere with the occupation, and is most worrying at night. The treatment, which consists of hygienic measures combined with friction, is efficacious and seems to point to the cause as lying in an extra accumulation of effete epidermic cells.

In *psoriasis* the spots are covered with a more yellowish, less shiny crust than in the white. When punctate the crust is thicker than usual and is easily rubbed off. It then leaves a deep red, bleeding surface, the blood oozing freely from the minute vessels. As in other diseases of this race in which there is a sufficiently thick amount of epidermis removed, a white spot remains for a while after healing. In the orbicular form—as in one of the cases reported—many of the larger spots could still be seen after many years. It is not a common disease of the black and is easily cured. I have seen no relapses, nor had there been previous attacks as far as could be found out.

Scabies. Most of the cases of scabies which come to my dispensary are either imported by emigrants or caught from them. As there is little intercourse between blacks and whites there is little chance of transferring the itch mite from one to the other. The four females reported here thought they caught it from washing the soiled clothes of white people. The two men had just returned from oyster-dredging on the Chesapeake Bay, where whites and blacks are mixed promiscuously and where ignorant foreigners are enticed before they know what a terrible life is before them.

The lesions produced by the itch mite in the negro are modified when compared to the whites. There is less inflammation, less excoriation, and the number of burrows fewer and shorter than in the white.

Seborrhœa capitis is a common disease of negro women, and is so common that they very seldom consult a physician for it. If, however, the heads of black women who present themselves for other troubles be looked at, it will be seen, according to my experience, that about fifty per cent. of them have a pityriasis with a collection of yellowish-looking crusts upon the scalp, with a line marking the limit of trouble at the edge of the hair growth. Want of cleanliness is the cause, and it is in such heads that pediculi are found in the negro women.

Syphilis. This disease in the negro offers many striking points of difference when compared with it in the whites, but time will hardly allow of more than the noting of a few salient points at present. It is a subject worth an especial study at some future day.

The time of incubation, or the time between the exposure and the appearance of the chancre, is generally less than in the white. The chancre is larger, more indurated, less painful, more apt to produce phimosis—if in the proper situation—and remains longer. The inguinal glands become more enlarged and are more prone to suppuration. The secondary eruption is most often macular, sometimes papular or tubercular, rarely orbicular or ulcerous. The eruption may easily be overlooked, as there is no redness. The spots are slightly lighter in color and sometimes resemble closely the lesions in herpes tonsurans maculosus. Accompanying the eruption the patients complain of an itching and usually there are signs of scratching present. The mucous membrane of mouth, nose, and throat is usually diseased, and the lymphatic glands universally enlarged.

The tertiary stage is more sure to follow, as the negro is subject to nodes, gummata, chronic ulcers, and the various other syphilitic sequelæ. In summing up its usual course in the negro I should say the disease was more prompt in its first stage, less violent in its second, and more obstinately lasting in its third.

Tinea tonsurans will be seen, from this record, to be common with the negro, as it numbers over eight per cent. of all. The disease, whether in ring form or spots, differs from that in the whites in not having a surrounding red color. The scales appear more white upon the pigmented surface. There is slight itching and it seems to be more easily conveyed from one negro to another, or, at any rate, it runs through a family of them more promptly. Treatment is, nevertheless, sure, quick, and satisfactory.

Urticaria in the black negro is a mild disease. The wheals are not very distinct, not much raised, there is no redness in their periphery, their centres are lighter in color than the rest of the skin. There are some burning and itching, but the linear stripes, so easily produced in whites by the nail, were not visible in the cases here reported.

Verruca.—Warts are common among the blacks. They are usually large papillomata, which must be energetically treated to be got rid of. They seem less frequent than fibroma in this race.

There is only one more disease which I wish to mention, and that is *dermatitis venenata.* It will be seen by the chart that there are no cases reported of it in the negro. I have not yet seen a case of what is called poison oak in this race. Several times I have asked country negroes if they have ever seen colored persons poisoned with poison oak.

The answer has been, "Yes, sah; lots of them." When asked if there was any eruption, they said: "No, sah; der isn't any ruption, but der's an awful eetching in dee blood."

Now, from what I have seen of negroes, I do not doubt that many times the itching is the only symptom which indicates any trouble of the black skin.

Not to carry this simple *résumé* too far, I wish only to remark that it seems to me probable that the pigmentation of the skin is the prime factor in causing the diseases of the skin in this race to differ from those of the white.

Whether any practical result in treatment may be obtained by following out and confirming this supposition must be left to a larger experience and to still closer observation.

HYPNOTISM AS A THERAPEUTIC AGENT.

BY G. J. PRESTON, M.D.,
OF BALTIMORE.

HYPNOTISM, using the term to embrace the various states known as mesmerism, braidism, magnetism, etc., has passed through a great many phases. Omitting, for the sake of brevity, its application by the religionists many centuries ago, we find Mesmer using it as a therapeutic agent. In 1779 he published his book, *Mémoire sur la découvêrte du magnétisme animal,* in which there were many absurd theories and unsubstantiated facts. Shortly after this the Société Royale de Médecine made a report on the subject, condemning it in very strong terms. Mesmer, however, had a number of followers, one of the most illustrious of whom was Puységur, who continued to investigate the subject in the half light in which his master, Mesmer, had worked.

Without mentioning the names of many who, during the next half century, were enthusiastically studying hypnotism, we pass to Braid, of Manchester, who, in 1841, definitely formulated the doctrine of suggestion. He repudiated entirely the "magnetic fluid" theory of Mesmer and his school, and, while he wandered a little into the then popular doctrine of phrenology, in the main his investigations were scientific and correct. In 1848, Grimes, of New England, and, in 1850, Dr. Dods, also an American, were active promulgators of this science. Almost all these men were physicians, and hypnotism was constantly employed for therapeutic purposes. Mesmer reports numbers of cases cured by this means. Unfortunately, there was so much of error in his conception of the subject of hypnotism, and his enthusiasm, amounting to a sort of fanaticism, was so great that it led him to apply his newly discovered remedy to every class of cases. Still he reports cures, of the verity of which there can be no doubt.

Braid treated many cases of hysterical paralysis, contractures, and the like, by what was really suggestion. Not understanding thoroughly the nature of suggestion, he was accustomed to practise a sort of massage upon the affected limb, while the patient was hypnotized, thus, of course, making a suggestion, which can be done as well by a simple affirmation of the hypnotizer. At this same time Dr. Esdaile, an English surgeon in the hospital of Calcutta, was employing hypnotism very generally. He wrote to Braid that in six years he had done more than six hundred capital operations while the patients were under the influence of the magnetic sleep. So much for hypnotism in the hands of its early advocates.

While interest in the subject flagged very greatly, it was never entirely lost, and from the early periods of the discovery of the science, if it may be so called, until the present, there have not been wanting faithful students of its phenomena. The revival of hypnotism which took place in France a few years ago, like most revivals, has been carried on with very intemperate zeal. Statements have been made as to the wonderful power of suggestion, and so-called facts put on record, that are quite worthy the day when hypnotism was regarded as a mystic art, containing something of the supernatural. One need only recall the recent experiments of M. Luys, of suggestion at a distance—as, for example, obtaining the intoxicating effects of alcohol by means of a small phial held near the hypnotized subject. Such

a fact as this, for it is a fact well attested by many observers of M. Luys's experiments, can only be explained by suggestion. M. Luys was, in all probability, himself deceived, for his subjects most certainly have had suggested to them the effects that were expected from the various drugs employed. Moreover, few or none of the many enthusiasts have been able to obtain any like results.

Before taking up the special therapeutics of hypnotism, it is necessary to speak of a very important difference between the two great schools of hypnotism, the one at Nancy, the other at Salpêtrière. M. Bernheim, of the Nancy school, has been experimenting for years with hypnotism, and has used many thousand subjects, and he comes to the conclusion that it is possible to hypnotize nearly every individual, if one will only patiently repeat the attempts from time to time. He thinks that hysteria has nothing to do with hypnotism, and, as proof of this assertion, great numbers of his experiments have been made upon men. In other words, his doctrine is that hypnotism, in its simple, uncomplicated state, must be studied only in the healthy subject. M. Charcot, and the school of the Salpêtrière, on the other hand, have shown the close relationship between hysteria and the hypnotic state, and hold that all persons in whom the hypnotic state can be brought about are hysterical, or come under the general term hysteria, the limits of which are very wide. They attest, as evidence, that many patients brought to the Salpêtrière with supposed organic affections, are stated by relatives and physicians to be absolutely free from hysteria, and yet careful observation detects undoubted signs of hysteria; the affection is treated accordingly, and cured. In addition, many cases of very well-marked hysteria have been described in men, and there are at the Salpêtrière now several men with hysterical hemianæsthesia.

It is very evident that the decision of this question, which requires more careful investigation, will have a very important bearing upon the subject of hypnotism. If, to obtain hypnotism in its most perfect form, the subject must be hysterical, then the application of this remedy is necessarily limited. Moreover, as a direct consequence of this position, it follows that the only conditions in which hypnotism can be of any service are purely affairs of the imagination, purely hysterical.

One of the earliest uses of hypnotism was to produce a state of insensibility, so that surgical operations could be performed without pain. The cases reported by Esdaile, mentioned above, and numerous instances related by other observers, show what an important part hypnotism would have played had it not been for the discovery of anæsthetics. Of course, after this discovery, the application of this agent fell into disuse, for several reasons, the most important being that not all persons were hypnotizable. Of late years, however, it has been employed for this purpose, especially at Nancy, and it has also been resorted to in accouchement.

As a means of confirming diagnosis, hypnotism is very valuable. Cases of paralysis, and often hysterical paralysis, counterfeiting most perfectly the organic affection, can be cleared up most wonderfully, oftentimes, by hypnotizing the patient. The same is true of contractions and of that very curious and interesting condition, hystero-epilepsy. It is a mistake to believe that this latter affection is confined to France; while it is probably seen in its highest perfection in Paris, it undoubtedly occurs in all other countries, notably in America. After having seen *la grande hystérie* at the Salpêtrière, any one who has seen much practice in the United States can recall such cases. Sometimes it is extremely difficult to make an exact diagnosis in this class of cases, for the first stage of the affection is very like ordinary epilepsy, but careful observation will always bring out the hysterical element.

Hypnotism offers a means of cure in those cases, or at least a means of preventing the attack. A great many hystero-epileptics know when the crisis is approaching, and if they are hypnotized and allowed to sleep for some hours the attack is generally averted. Unfortunately, it is not always possible to bring them under this influence at this time. Often there seems to be a sort of exaltation which effectually prevents the use of hypnotism. The patient will try to enter into this condition, for he dreads the crisis of hysteria, but will often say "it is impossible for me to sleep." Still it is possible, in a large number of cases, thus to intercept the attacks, which will, when treated in this manner, grow more and more rare.

Cases of monoplegia, contractures, and the like are very amenable to hypnotism. The treatment of this class of cases is that by suggestion. Just how far it is possible to go in this direction, we do not as yet know. If it is possible to produce vesication, and this has been repeatedly done on very susceptible subjects, one can hardly say where the limits should be fixed.

One very important point that the study of hypnotism has brought out and emphasized, is the potency of suggestion. Some light is let in upon the influence that the charlatan possesses over the people in general, and the faith in patent medicines and in homœopathy meets an easy solution. Doubtless most of the slight aches and pains that the general practitioner is called upon to treat are partly imaginary, and all that is necessary for cure is a certain amount of faith on the part of the patient, begotten by judicious suggestion by the medical man. At first sight, this seems to be a sort of chicanery, but it is impossible to deny its efficacy, and it is much safer for the doctor to

acknowledge, to himself at least, that it is not his simple remedy which has wrought the cure, but his suggestion to the patient.

One needs no better example of the power of suggestion than the many cures brought about by the *faith-cure*. In this case religious fanaticism is called upon to produce an effect upon the mind of the credulous patient, and if the malady is an imaginary one the relief is instantaneous. It was thought by some of the advocates of hypnotism, that it would prove an important agent in the treatment of certain forms of mental disease, especially paranoia; but such expectations have not been realized. In the first place, it is extremely difficult to hypnotize the insane; and, in the second place, the delusion has become so fixed, at least when the patient is in an asylum, that it is practically impossible to dislodge it. However, several very interesting cures have been reported, particularly those of M. Voisin in his wards at the Salpêtrière. These cases were evidently what might be called hysterical mania. Doubtless it would be quite possible to treat successfully certain cases of beginning paranoia before the delusion has become permanently fixed, and while it is merely an insistent idea.

It is possible in a short paper only to indicate the therapeutics of hypnotism, which are capable of very considerable elaboration. That hypnotism has its uses no one who has followed the careful and systematic work that has been expended upon it during the past few years can deny. On the other hand, it has certainly not met the extravagant anticipations that were raised a few years ago, when it was first employed as a therapeutic agent. Future investigation is needed to fix exactly its place in the category of remedial agents.

A word of warning must be given, as is necessary in the case of all therapeutic agents. Without doubt, harm can be done by the injudicious use of hypnotism. Already a number of cases of confirmed hysteria and even of complete mental derangement have been recorded in France, the results of the employment of hypnotism by persons unfit to undertake such experiments. Like other remedies, it should be used only to counteract some abnormal state, and its use as a parlor amusement cannot be too highly condemned.

PARIS, September 27, 1888.

MEDICAL PROGRESS.

Methyl Chloride as a Local Anæsthetic.—According to BAILLY, if a stream of chloride of methyl is poured upon some dry cotton, held in silk, a temperature of —9° to —67° is produced, which persists for a quarter of an hour to an hour. Passed by the forceps over the skin, it produces temporary analgesia sufficient for a number of minor surgical operations, and may be used to relieve certain painful conditions, such as peripheral neuralgias; in muscular rheumatism, particularly in lumbago and torticollis, have striking results been obtained. Gastralgias, hepatic colic, even a case of tetanus, were favorably influenced by the remedy, which the writer designates "stypazi."—*Deutsche med. Wochenschrift*, September 6, 1888.

Capsules Analgesiques.—

℞.—Menthol,
Antifebrin . . . . āā ʒj.
Sacchari lactis . . . q. s.—M.

Divide in capsules No. 24.

The dose is one or two capsules.—*Revue de Thérapeutique.*

Helleborein as a Local Anæsthetic.—VITTORIO and ELOIDIO (*Allg. med. Centralzeitung*) have found that three or four drops (gr. $\frac{1}{40}$th to gr. $\frac{1}{30}$th) of a watery solution of helleborein, instilled into the conjunctival sac of the eye of rabbits or dogs, in about ten minutes produce complete anæsthesia of the cornea for half an hour, the other contiguous portions of the eye retaining their sensibility. Though helleborein may possess some advantages over cocaine, the extraordinary toxicity of the former will stand in the way of its frequent use. Permanent changes and unpleasant complications, such as manifestations of irritation in the eye, which are, for instance, to be feared from erythrophlæin, have not yet been observed from the use of helleborein.—*Münchener med. Wochenschrift*, September 11, 1888.

Hot-air Inhalations in Phthisis.—Gratifying results have recently been obtained in treating phthisical patients with inhalations of hot air, by Weigert, an American resident in Berlin. He found that tubercle bacilli outside the body die at a temperature of 41° C., and are adversely affected by one of 38°, and had constructed an apparatus for the inhalation of heated air, and commenced to make trials on phthisical patients in the early stage recommended to him by other medical men, he himself not being in practice. At first a temperature of from 40° to 60° C. was employed, the air for inhalation being quite dry. This temperature was gradually raised as high as 80° C. The patients bore this hot dry air exceedingly well, and continued to inhale it for three or four hours a day during a month, the only unpleasant effects produced being hyperæmia and dryness of the mucous membrane. The general effects are represented as having been remarkable, patients who had been falling away picking up strength and becoming quite robust, the physical evamination showing at the same time that the dulness and râles had perceptibly decreased. The bacilli in the sputum, which had been very numerous, rapidly diminished in number, and finally disappeared altogether. These observations were confirmed by several other medical men. Dr. Halter, of Lengerich, Westphalia, seems to have gone even further than Dr. Weigert, he having himself inhaled, and caused patients also to inhale, dry air heated to 190° C., with satisfactory results.—*Lancet*, Sept. 29, 1888.

Sand-bags for Hemoptysis.—In a case of tubercular hemoptysis, the hemorrhage coming from the left apex, BETZ, of Heilbronn (*Memorabilien*), attained prompt

results from the application of a two-pound sand-bag over the left apex, after he had observed that the respiration was greatly quickened, 33 to 35 per minute, and particularly that there was greater expansion in the left subclavicular region than in the right. The pressure of the weight reduced the number of respirations and aided the breathing. The hemoptysis ceased within twenty-four hours.—*Münchener medicin. Wochenschr.*, Sept. 11, 1888.

Treatment of Laryngeal Phthisis.—A solution of menthol, twenty parts, in one hundred parts of olive oil is much used by BEEHAG, with good results. By means of a syringe, prepared for the purpose, he applies it directly to the mucosa of the larynx, guided by the laryngoscope. At the moment of inspiration, the patient inspires deeply and particles of the medicament are carried into the finer bronchi. The cooling and anæsthetic effects of the menthol are at once developed, and the plan is devoid of any disagreeable effects.—*L'Union Médicale*, September, 1888.

To Keep Hypodermatic Syringes Clean.—Hypodermatic syringes, the canulæ of which have become obstructed, may be cleared by holding the needle for a moment over a flame; the foreign matter is at once expelled or destroyed. Should the wire in the canula become rusted, the former can be removed by dipping the latter in oil and holding it over a flame. It is well to introduce oil into the needle and then to heat the latter; rusting is thereby prevented. When about to use the needle, alcohol should be passed through it.—*Münchener medicin. Wochenschr.*, September 11, 1888.

Eczema of Anus and Genitals.—LUSTGARTEN advises hot sitz-baths, rendered very soapy, and, subsequently, the application of the following pomade.

℞.—Oleate of cocaine . . . 1 part.
Olive oil . . . . . 2 parts.
Lanolin . . . . . 10 " —M.

Apply twice daily to affected parts.—*Le Progrès Médical.*

Amaurosis from Dental Caries.—RÉVA (*Revista de Ciencias Med.*) reports a case of complete amaurosis, in a woman, aged thirty, upon the side of a carious second molar teeth of the upper jaw. Upon the removal of the diseased tooth, in the root of which a fragment of a wooden dental drill was found, sight returned.—*Medicin. Chirurg. Rundschau*, August 1; from *St. Petersb. med. Wochenschr.*, No. 21, 1888.

The Treatment of Ingrowing Toe-nail.—HOFMANN (*München. med. Wochenschr.*, No. 32, 1888) recommends the following procedure: The wound and the part are thoroughly cleansed and disinfected with sublimate solution; several drops of a solution of ferric chloride are poured upon the sore spot and the nail elevated; the part is then permitted to dry. This process is repeated on the second and third days. In a few days the hard crust which has formed is removed with the forceps. The bleeding which results is checked by the use of the iron solution. In three or four days the crust is again removed; and this is repeated until the surface of the skin is smooth. On the second or third day, already, the nail, softened and made friable by the iron solution, may, with some care, be painlessly removed with a small scissors or blunt knife. To prevent recurrence, bits of cork should be placed under the margin of the nail.—*Therapeutische Monatshefte*, Sept. 1888.

Rumination in Man.—A case of rumination in man has been observed by a physician of Odessa, occurring in a man of twenty-two years. The food was regurgitated about fifteen minutes after eating, and the chewing continued for some time, often for several hours. The patient, who was married, had been addicted to excessive sexual intercourse for three years, to which fact the regurgitation was attributed. It seemed to be a case of marked sympathy between the genital, gastric, and œsophageal functions.—*Revue Générale de Clinique et de Thérapeutique*, September 20, 1888.

Treatment of Ascites.—Although not a new device, the treatment of ascites by faradization is again brought forward by MURET, who reports good results in some of his own cases. He applies the current directly to the abdominal walls, daily, for several months, and finds that muscular contraction induces greater activity in the lymph channels and consequently absorption of the fluid.—*Revue de Médecine*, September, 1888.

The Administration of Creasote.—KEFERSTEIN recommends the following formulæ as useful for the administration of creasote:

℞.—Creasoti . . . . 1.3 parts.
Spts. vini. rectif. . . . 25 "
Aqua cinnamomi . . 100 "
Syrup. cinnamomi . . 25 "—M.

S.—A tablespoonful three times daily; increasing one tablespoonful weekly.

℞.—Creasoti . . . . . ʒ j.
Pulv. rad. altheæ . . . . ʒ jss.
Succi liquiritiæ . . . . ʒ jss.

Mucilag. acaciæ q. s. ut ft. pil., No. 120, obduc. gelatin.
Sig.—Six pills three times daily.

℞.—Creasoti . . . . ♏ xv.
Plumbi acetat.,
Pulv. opii . . . . āā gr. iv.
Succi liquiritiæ . . . ʒ jss.

Mucilag. acaciæ q. s. ut. pil., No. 50.
Sig.—Five pills three times daily.

For cough and diarrhœa:
℞.—Creasoti . . . . 1.3 parts.
Solve in
Ol. amygdal. . . . 30 parts.
Pulv. acaciæ . . . 20 "
Aquæ dest. . . . . 100 "—M.
Ft. emulsio; adde
Tinct. aurantii comp. . . 1 part.
Elæosacchar. menth. pip. . 4 parts.—M.

Sig.—A tablespoonful three to five times daily.—*Therapeutische Monatshefte*, September, 1888.

Alcohol and Cirrhosis of the Liver.—M. ALISON, after an exhaustive consideration of the effects of alcohol on the economy and its special action on the liver, believes that the business of the individual, outside of the causes commonly attributed to cirrhosis, plays an important *rôle* in its development. Field laborers are rarely attacked by it, and it is more rare in persons given to manual labor than in those who lead sedentary lives. He advises physical training as an effective therapeutic means.—*Archives Générales de Médecine*, September, 1888.

The Treatment of the Atonic Leg Ulcer.—APPENRODT recommends the treatment of atonic ulcers of the lower extremity by massage. For some days before manipulation is commenced, the sore is treated antiseptically and the entire leg made aseptic, for otherwise abscesses and furuncles would result from infectious matters being forced into the lymph channels. At first, gentle massage is performed below the knee and gradually advanced upward. The manipulation drives the lymph from the excoriated surfaces—an evidence of success. Lanolin is used to anoint the skin. Before and after the massage, the skin is thoroughly cleansed and disinfected. The ulcer and the surrounding inflammatory area are covered with gauze spread with lanolin, over which are placed tissue-paper and a bandage. The addition of disinfectants to the lanolin is not only unnecessary, but in most cases harmful. Only when the skin is not susceptible may iodoform gauze be used, because this often gives rise to erythema. Impermeable coverings are to be avoided. Swelling and hypersecretion disappear and granulations soon show themselves. Rarely is it necessary to add starch or zinc oxide to the lanolin to overcome any eczema. The massage must be continued after the ulcer has healed and the limb be judiciously used.—*Therapeutische Monatshefte*, September, 1888.

Administration of Morphine.—It is pointed out in *Les Nouveaux Remèdes* that morphine should never be given in cherry-laurel water, or in any solution containing hydrocyanic acid, however small the quantity. The precipitate formed of morphine and the acid is a most deadly poison. It forms slowly, and the patient is liable to take a toxic dose when the contents of the bottle are nearly exhausted.

The Treatment of Retro-pharyngeal Abscesses.—BURCKHARDT, of Stuttgart, in three cases, has successfully opened retro-pharyngeal abscesses by incisions into the neck. The skin and platysma, at the level of the larynx, are divided by an incision parallel with the anterior border of the sterno-mastoid muscle, the cervical vessels pushed outward and dressing forceps introduced between the latter and the larynx toward the abscess. The advantages of this method consist in the possibility of anæsthesia, ready drainage, and antiseptic treatment of the cavity.—*Deutsche med. Wochenschrift*, September 13, 1888.

Rupture of the Spleen.—M. BARRALLIER, who has made a study of this accident, concludes that it is most liable to occur in the earlier stages of malarial poisoning, when the spleen is suddenly inflamed and the seat of great vascular engorgement. The lesion, which is commonly fatal, has a sudden onset, and is preceded by no special symptoms. He designates it as a pernicious splenic fever.—*Archiv. Gén. de Méd.*, Sept. 1888.

For Itching.—To relieve the itching of hemorrhoidal affections, pruritus ani, pruritus senilis, etc., wash the part with lukewarm water and good soap; then rub in lanolin in the following combination:

℞.—Lanolin puriss. . . . 30 parts.
Vaselini,
Olei olivæ . . . āā 20 parts.

When the itching about the anus is severe, cocaine may be added thus:

℞.—Cocainæ hydrochlorat. . $\frac{1}{10}$ to $\frac{1}{5}$ part.
Lanolin puriss. . . . 30 parts.
Vaselini,
Olei olivæ. . . . . āā 20 "

The addition of ten per cent. of flowers of sulphur has proved useful.—*Therapeutische Monatshefte*, September, 1888.

Antiseptic Obstetric Dressing without Injection.—In view of the fact that intra-uterine injections are by no means devoid of danger, ROULIN practises and recommends that just previous to labor the external genitals be thoroughly cleansed with a solution of chloral, 1 : 100. After the birth of the child the parts are again washed and the vulva covered with several layers of lint sterilized and impregnated with salol. Over this is placed a cotton wad, wet in a solution of chloral of the same strength as that before mentioned, and wrung out nearly dry. The dressings are changed every six hours. Eight cases have been treated in this way with excellent results. The originator admits that in cases of retained placenta and the like, injections would be required, but ordinarily this plan answers admirably and is wholly free from danger.—*L'Union Médicale*, September 27, 1888.

Bandaging the Extremities for Pulmonary Hemorrhage.—SEITZ (*Archiv für klinische Medicin.*), as the result of experimental investigations, recommends bandaging the extremities for hemorrhage from the lungs; first the upper extremities, at or about the middle of the arm, then the lower at the middle of the thigh or immediately below the knee, using silk bands about three-quarters of an inch wide; or, in case of necessity, strips of any sort. By imprisoning the blood, the tension of the left ventricle is diminished, and contraction of the vessels in the area not included occurs. The half hour during which the bandages are retained suffices for the formation of a thrombus at the site of hemorrhage. The method was used in the time of Hippocrates.—*Deutsche medicin. Wochenschr.*, September 20, 1888.

For Hysterical Hyperæsthesiæ, Gastralgiæ, Vomiting, and Spasmodic Conditions.—EWALD (*Digestive Disorders*, vol. ii.) recommends the following:

℞.—Morphinæ hydrochlorat. . gr. iij.
Cocainæ hydrochlorat. . gr. iv–viij.
Tinct. belladonnæ . . ʒ j–ijss.
Aquæ amygdal. amar. . ʒ vj.—M.

Sig.—Ten to fifteen drops hourly.—*Therapeutische Monatshefte*, September, 1888.

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SATURDAY, OCTOBER 20, 1888.

THE CASE OF FREDERICK THE NOBLE.

THE arraignment of Sir Morell Mackenzie by the German physicians associated with him in the medical management of the unfortunate Frederick the Third, and principally by Gerhardt, von Bergmann, and Landgraf, has received from Mackenzie a scathing rejoinder, which contains a distinct accusation that von Bergmann, in futile endeavors to replace a tracheal canula, made a false passage in front of the trachea, which was the immediate cause of precipitating the inevitably fatal result.

We have refrained from commenting upon the report of the German physicians, although in our possession several months, until we should have read the report of Mackenzie. In general terms, we may state that the questions raised in this unseemly controversy are largely questions of veracity;—questions which cannot be decided here, where Mackenzie and his chief opponents have always been held in equally excellent repute. We decline to enter into the details further than is necessary to present a strictly clinical aspect of the case itself as gathered from the two reports.

There are three important clinical points involved, which are of great scientific and therapeutic interest. All the others seem foreign to the main features of the case, being contingencies and complications which might occur under various conditions, or might not occur at all; and we shall not discuss them. These three points may be put in the form of questions.

1st. Was it at all probable that a benign growth was practically transformed into malignancy by the applications made with the electric cautery, before a fragment of the growth had been removed for microscopic examination, or by the manipulations subsequent?

2d. Is it probable that thyrotomy and eradication or radical destruction of the growth *in situ*, at the time that preparations had been made for the purpose, would have been more successful than the plan adopted?

3d. Was unilateral or complete excision of the larynx justifiable at any period of the malady, and if so, at what particular period?

First. A thorough and painstaking collective investigation, now perhaps completed, by Dr. Felix Semon, of London, has been so far published as to show great improbability that intralaryngeal manipulation has ever converted a benign tumor into a malign one; and that it is quite likely that the ultimately malign growths were malign originally, even in the few instances in which the transformation was believed to have taken place. In view of the conclusions drawn from this collective investigation of several thousand benign neoplasms submitted to intralaryngeal manipulations the most diverse, it is strange that so well informed a laryngologist as Sir Morell Mackenzie could have done more than hint at the remote possibility of such an occurrence; the more so as his own manipulations were by no means free from the same implication. Even those laryngologists who had hitherto held this view the most strongly, have corrected their previous opinions. This question is a very important one; for if the answer should be affirmative as to more than an insignificant percentage, the inference would follow that repeated efforts at intralaryngeal procedures for the eradication of benign neoplasms would have to be superseded by procedures for eradication by external access. In the instance under discussion, the fragments removed by Mackenzie and pronounced benign by Virchow, must have been the very surfaces most likely to undergo the transformation alluded to. We, unhesitatingly, answer the first question in the negative.

Secondly. At an early period in its history, a neoplasm, as yet insignificant in its visible dimensions, occupies a portion of the larynx more frequently the special seat of malign growths than of benign ones. It exists in the person of a man in his prime and apparently in the best of general good health. The

patient's age and a partial family history of cancer render the case at least suspicious. It must be closely watched. Failure to control the development of this neoplasm by intralaryngeal attack, instituted under either the hope or the conviction that it was benign, leads to the opinion that it is not benign; and intralaryngeal interference is thereupon suspended. This is the situation when thyrotomy is proposed.

The operation of thyrotomy to afford complete access to the neoplasm for the purpose of eradicating it in a few minutes, provided it appeared that it was limited to a small portion of the interior of the larynx, was, in our opinion, a very proper proceeding under the circumstances—in the light of the subsequent clinical history probably the very best, whether the tumor should prove to be benign or malignant. If the latter, the importance of having had the proposed operation performed at the earliest moment practicable, cannot be successfully contested.

Thyrotomy, in itself, is not a dangerous operation. Performed at a time of election, it is really less dangerous than tracheotomy performed in an emergency. A healthy subject is rarely confined to his room for more than two or three days after the operation; and he is often able to leave his house in from a week to ten days. If, however, a large and vascular neoplasm occupies the interior of the larynx, its extirpation may be attended with copious hemorrhage, and inflict such injuries to the tissues as may compromise the result. As far as may be judged from all the reports, no such complication was to be anticipated; and the probabilities are, therefore, strong that the immediate result of this operation would have been a good one, although the voice would have been impaired. It could hardly have been worse than the plan adopted. It might have been salvation. Should the tumor have proved to be benign, a permanent cure would have been in immediate prospect. Should it have proved to be malign, the prospect of recurrence would have remained for some time in doubt; probably for a long time, provided that the whole of the morbid growth had been removed with a sufficiently extensive zone of healthy tissues. Should it have turned out that the growth was not only malign, but that it extended beyond the field visible on laryngoscopic inspection, the question of resort to radical measures by excision of the larynx, in whole or in part, would have had to be settled at once, instead of being postponed to a period which, under symptomatic and expectant treatment, would render the prospects of recovery still more problematical. In a case of malign growth, the sooner a decision is reached in this respect the better.

In estimating the probabilities of death from the operation of thyrotomy in carcinoma of the larynx, or in gauging the prospects of recurrence, a great deal depends upon the stage of the disease at which the operation is performed. Each case must be made a study in its own behalf. Statistical computations are often misleading. Sir Morell Mackenzie gives a list of twenty-two thyrotomies for carcinoma, in which there are only two cures.

We have not the data at hand to determine the stage of disease at which these operations were undertaken; but our belief is that not one of them was undertaken at a period so promising for success as the case in question at the time the operation was contemplated;—hence the inference that in this instance another case would probably have been added to the series of cures, rather than to the series of deaths. We notice, however, that Mackenzie has failed to include two cases of cure, one of three, and the other of twenty years' standing, both mentioned on page 20 of the pamphlet he criticises, and from pages 19 and 21 of which he makes citations at the very head of his table. The first case alluded to was one of von Bergmann's, which, he states, he vainly requested Mackenzie to examine. The other is one of the elder Solis-Cohen's, performed in February, 1868, the subject of which is still living. This case was reported in THE MEDICAL NEWS of December 3, 1887, and was reproduced in the *Revue Mensuelle de Laryngologie*, etc., 1882, No. 2. In another instance in the thyrotomy table, of which we have cognizance, recurrence did not take place in three months, as recorded, and did not take place in the larynx at all. An independent epithelioma became developed in the cutaneous cicatrix.

To our mind, the minute size of the original growth in the Emperor's case, and still more the facts that Mackenzie states that he destroyed it so utterly that by August 13th the last slight wound of his electric cautery had healed, and that the growth thus destroyed never returned, are proofs incontrovertible that this same growth, which required three months for its eradication by intralaryngeal procedures, could have been removed in a few minutes, just as radically, by thyrotomy, on that

memorable 21st of May, when everything was ready for that purpose. In this connection we cannot refrain from expressing our surprise that the two slight cauterizations, described by Mackenzie, accomplished so much. It adds another unique feature to this remarkable case.

It should be remembered, however, that the growth pronounced benign by Virchow, and destroyed by Mackenzie, may have been really benign; and that the malignant neoplasm existed beneath it and out of sight; and, in further support of Mackenzie's position, it should also be remembered that Virchow distinctly stated that his studies in this connection had failed to reveal a single example of coexistence of benign and malignant growth.

The light now shed on the case enables us to answer the second question in the affirmative.

The *third* question is much harder to answer; and here our judgment leans much further toward the views maintained by Mackenzie. That life is in imminent immediate peril in every case of complete excision of the larynx, there is but little doubt. There is but little less doubt that it is nearly as practically imperilled in unilateral laryngectomy. Under both conditions, however, the stage of the disease is of immense importance. The better selection of cases is rendering both operations more and more promising of success. Few patients die from the immediate effects of either operation; but a large number perish within two weeks from pneumonia. A great many perish from recurrence at a period anterior to that which they have a prospect of reaching under expectant treatment. A few are cured.[1]

The choice of the two alternatives: inevitable death at a period varying say between one and three years from the beginning of the malady; and a forlorn chance of recovery should the immediate and the immediately proximate risks be successfully surmounted, must in great measure be left to an intelligent patient himself. There are occasions under pressure of which a man might well prefer a certainty of life sufficiently long to provide for his family, or to attend to some matters of great humanitarian importance, rather than run the risk of almost immediate death with a bare chance of ultimate recovery. There may be circumstances under which a physician might be debarred from communicating such a decision to others; and be compelled to bear the entire responsibilities that may ensue in consequence. The decision once made, every day of delay lessens the chance of success, and thus, in a very short time, the result of a decision not to submit to any radical operation becomes irretrievable.

A careful review of both stories has led us to the opinion that, thyrotomy having been abandoned at the commencement, a radical operation upon the larynx was justifiable up to the period at which Landgraf is said to have noted extension of the morbid process and its commencing encroachments upon the hitherto sound side, and to have urged a consultation in consequence; but after that period the radical operation was no longer justifiable.

Our answer to the third question is, that, if the patient were willing, a radical operation was probably justifiable during a portion of his sojourn in Great Britain, and that after his removal to San Remo the time for such measures had passed.

[1] Park's case, 1885, "reported cured in six months" in Mackenzie's table, remains a CURE, and is still alive (1888) without recurrence.

THE Trustees of the Hoagland Laboratory, in Brooklyn, announce the completion of the Laboratory and its equipment for practical work Special facilities are offered to those who desire to prosecute original research. For this purpose private laboratories have been provided, and arrangements are now being made for the purchase of a library which shall contain all the literature necessary for reference in the departments of bacteriology, physiology, and pathology.

Owing to the absence in the South of Dr. George M. Sternberg, the Director, in the further prosecution of his investigation into the cause of yellow fever under orders from the President of the United States, the course of lectures on bacteriology, already announced, will be postponed until his return. The Trustees further announce that the services of George T. Kemp, Ph.D. John Hopkins University, have been obtained as Associate in bacteriology and physiology, and that, with his assistance and under the direction of Dr. Sternberg, practical instruction in bacteriology will be given during the Winter and Spring.

DR. WILLIAM T. LUSK has been elected President of the New York State Medical Association.

DR. OSLER does not leave Philadelphia to enter upon his duties as physician to the Johns Hopkins Hospital until next May.

DR. JOHN N. MACKENZIE has been elected Clinical Professor of Diseases of the Throat and Nose in the Medical Faculty of the University of Maryland, *vice* Dr. Frank Donaldson, resigned.

ZUCKERANDL, formerly of Grätz, has been made Professor of Anatomy at the University of Vienna; von Ebner, also formerly of Grätz, Professor of Histology; and Puschmann, Professor of the History of Medicine.

SOCIETY PROCEEDINGS.

ASSOCIATION OF AMERICAN PHYSICIANS.

Third Annual Meeting,
Held at Washington, September 18, 19, and 20, 1888.

(Specially reported for THE MEDICAL NEWS.)

(Concluded from page 429)

SEPTEMBER 20TH.—MORNING SESSION.

DR. GEORGE L. PEABODY, of New York, presented the pathological specimens from the case in which there was

A PIN IMBEDDED IN A WOMAN'S HEART.

The patient died at the New York Hospital the 26th of last May. She was under observation only a few hours. On admission she was suffering greatly from dyspnœa and was unable to give a circumstantial account of her illness. For some time previously she had passed less urine than normal, and her feet had been swollen. She suffered from constipation, vertigo, spots before the eyes. Her appetite was poor, bowels regular. Temperature 100.8°, respiration 44, pulse 130. The specimen of urine was of a yellow color, acid in reaction, and of specific gravity 1.020, with albumen, epithelial casts, and pus cells. Her chest contained a large amount of fluid; moist râles both behind and in front. No murmur. Heart-sounds very feeble. Œdema of feet and legs. She was given acetate of potash and infusion of digitalis, and passed a fairly comfortable night. The next morning she seemed to be doing fairly well, when she suddenly experienced great precordial pain, distress, and intense dyspnœa. She was given morphia subcutaneously, but she continued to suffer until death came to her relief in half an hour.

At the post-mortem a black, eroded pin, the top of the head of which was free in the left ventricle, was seen piercing one of the papillary muscles attached to the anterior segment of the mitral valves, and penetrating the wall of the ventricle obliquely to the depth of a little more than an inch. The point of the pin was distant one-fifth of an inch from the external surface of the heart. There was no evidence of any recent inflammation in this neighborhood, but the endocardium about the protruding head of the pin was greatly thickened and snow-white, and firmly adherent to the edge of the head. A most careful examination failed to reveal the route by which the pin had travelled to its present location, and it is interesting that it should have remained for a long time free in the cavity of the ventricle without producing thrombosis. The pin had evidently been in its present situation a long time, and, in all probability, had entered the body by the mouth.

ON THE RELATION BETWEEN TROPHIC LESIONS AND DISEASES OF THE NERVOUS SYSTEM.

DR. E. C. SEGUIN, of New York, opened the discussion on this subject. He said that he excluded from consideration vasomotor disorders, and diffused or quantitative nutritional changes, partly because he considers them to be a distinct subject, and partly because their study would be beyond the scope of a short paper. He divided the so-called "trophic lesions" into two great classes. In one the lesions are probably mostly due to the action of extraneous causes, and are preventable. The second class embraces lesions which seem to be directly due to the nervous disease, and which are its necessary and unpreventable results. He endeavored to show that lesions of the second class, which alone merit the name of *trophic lesions*, occur in organs and tissues which are anatomically continuous, and whose life (or preservation of structure and function) is associated or interdependent. He concluded that disease of the nervous system produces true trophic lesions when it interferes with the associated or interdependent life of continuous tissues. This proposition he offered only as a partial and preliminary answer to the question under discussion.

DR. H. C. WOOD, of Philadelphia, as co-referee, presented the following conclusions: 1. It is physiologically proven that the nervous system directly affects general nutrition. 2. Various lesions are the immediate result of previous nervous disease. 3. In various cases the lesions are not preceded by circulatory disturbances. 4. No known vasomotor condition is capable of causing any of these lesions. 5. Therefore, it is absurd to attribute changes to preceding vasomotor changes.

DR. W. M. ORD, of London, said that he felt very much honored at being asked to speak on this important subject, but before he said a word he would beg to tender the Association his gratification at the very high tone and character of the discussions on the papers he had previously heard in this room.

As regards the two papers he had heard read, they had certainly stood in strong contrast with one another: the one critical, the other appealed strongly to the imagination and fascinated one by its strong and logical character of thought and imagery. He would try to prove at the start how, from disturbances of nutrition, there is a starting-point in the nervous system of the local hepatic disturbances. He did not think Dr. Seguin denied that the changes in chronic rheumatism are of a histological character. He would quote two cases bearing upon this point which he saw recently before leaving London, having had them under observation for more than a year. The first was a case of chronic hypertrophic cervical pachymeningitis, which presented a very typical form of that disease. This patient, a young man, at the time of his admission showed very little change in the skin and none in the joints. He was kept in bed, treated, and recovered to a large extent his motor power, and completely his sensation. There was a steady wasting of the tissue around the second and third digits, a distinct and marked change in the joints. The joints of the thumbs and fingers became somewhat tender, and finally loose. All the physical signs of progressive chronic arthritis developed in conjunction with dystrophy

of skin and muscle without any local irritating cause. The next case was an old man, who, for many years, lost power and wasted in the upper extremities. This man had a wasting of the skin, and a very remarkable change in the joints. They were considerably swollen, were constantly dislocated and as constantly put back in place. The whole muscles of the upper extremities were wasted. He sat up in bed with great difficulty. These two cases illustrate very well the primary affection of the nervous system in determining dystrophy of the joints.

Early in his endeavors to analyze pseudo-arthritis he was brought face to face with a class of cases which interested him very much. As a woman approaches the climacteric period one sees a painful swelling of the small joints of the fingers and toes. Examining this with great care the one thing always present was disorder of the catamenial function, and chiefly such a disorder as consisted in menorrhagia. He had seen cases in which, as the climacteric began to develop, the joint affections returned with intensified severity; but the day before he left London he saw two cases: one was a lady of sixty years, who had troubles of this kind during the climacteric period, and had a large uterine fibroid. Here was an unusually persistent type of the subacute form of this disease long after the cessation of the catamenia. People who suffer in this way are, in all cases, very excitable, anæmic, and sensitive women. It appeared to him that, as affections of the nerves or spinal cord may bring about neurotic dystrophies in joints, so local irritation may produce, in a reflex way, affections of joints more or less parallel to the affection determined by nerve disease. In the same way he has great reason to believe that continued prostatic irritation will produce affection in the joints of the leg. Gonorrhœal rheumatism may itself not be the result of a microbe, but the work of the nervous system. Acute rheumatism might be traceable to similar causes. It seemed to him that acute rheumatism is often developed under conditions which would indicate affections of the nervous system—such affections as would bring about, in a reflex way, the irritation of the joint. It occurs certainly after parturition. It occurs almost invariably in people who are very anæmic, or have lost large quantities of blood, and whose nervous system is brought into a state of over-expansiveness.

DR. M. ALLEN STARR, of New York, said that he had very little to add to the discussion. It seemed to him that the departure of Dr. Seguin marks a decided advance toward a certain amount of clearness in the study of trophic disturbances. The point he made, that trophic disturbances must be distinctly separated from nutritive disturbances, has not been appreciated, and it seemed to him to be a very important one, and one that threw considerable light on the subject.

There is a class of nutritive disturbances that has not been mentioned yet, which has been described by Dr. Osler as cerebral palsies, viz., that class of atrophies which follows hemiplegia in children. In adults it does not appear to be followed by any amount of atrophy on the paralyzed side. It is important to separate vasomotor from trophic disturbances. Trophic disturbances follow very closely on vasomotor disturbances, and it is not strange that the two should be confounded.

DR. H. P. BOWDITCH, of Boston, agreed entirely with Dr. Seguin that any attempt to explain the mechanism of trophic changes must be speculative in our present knowledge. We have to recognize in muscular tissues two distinct metabolic processes, one having its cause in the nitrogenous and the other in the non-nitrogenous metabolism. These are distinct inasmuch as it has been shown that the nitrogenous metabolism is not connected with muscular activity, while the non-nitrogenous is. When the muscular activity is kept within physiological limits urea excretion remains the same. Nitrogenous metabolism of the body is essential for muscular activity. It is essential for muscular activity that an abundant stream of nitrogen should flow into the body and out again. So the nitrogenous metabolism of the muscles goes on quite independently of its functional activity. If we limit the term trophic to the nitrogenous metabolism of the muscle and let the non-nitrogenous metabolism go as the functional activity, we would simplify matters a good deal.

DR. PYE SMITH, of London, said that he had listened with great pleasure to the two admirable papers read before the meeting. He confessed that both from a physiological and clinical standpoint his judgment would be entirely on the side taken by Dr. Wood with such eloquence and zeal. He did not quite agree with Dr. Wood in imputing a logical error to those who resort to the supposition of traumatism and vasomotor disturbances to explain the phenomenon in question. He would quote the case reported by Hilton of a patient with an ulcer in the hand which got worse and worse in spite of all endeavors to cure it, when it was discovered that a minute exostosis was pressing on the ulnar nerve, and when the exostosis was removed the ulcer healed rapidly. Gangrene caused by an embolus obstructing an artery is a well-known fact. He still thought there are cases of gangrene quite inexplicable on such an hypothesis. They had all seen those strange cases of skin eruption come on rapidly following lesions of the cord. His own experience is that these trophic cutaneous lesions are bullous or herpetic in character. He thought they must admit from such strong evidence that trophic nerves have a true position not only in physiology but in medicine.

DR. FERRIER, of London, was of the opinion that there can be no question of the direct influence of the nervous system on the nutrition of the tissues altogether apart from vasomotor changes, and agencies operating *ab-extra*. This is shown, among other things, by the structural modifications which are observed in the glands on stimulation of their secretory nerves; and by the degeneration which ensues in muscles which are separated from the cells of the anterior cornua, or when these latter are destroyed.

This degeneration he holds to be the direct result of the severance of the muscles from their trophic centres, and is not due to secondary inflammatory changes, inasmuch as it occurs under conditions which entirely obviate the initiation and propagation of irritation. But he believes with Dr. Seguin that the great majority of the trophic disturbances which so commonly occur in connection with injuries of nerves, such as panophthalmitis from lesion of the fifth, and the herpetic and other cutaneous disorders from lesion of common sensory nerves, are not due to the withdrawal of trophic influence, but are the results of secondary inflammation or injurious agents operating from without. They are observed more especially in connection with partial lesions of nerves and those most calculated to excite irritation.

He should, however, expect to find (although he is

not aware that the subject has been fully investigated) that section of all the sensory nerves of any given region would induce degeneration of the nerve endings and their annexes, precisely analogous in character to the degeneration which occurs in muscles when the motor nerves are divided, or the anterior cornua destroyed.

But the point is whether these and similar facts are to be regarded as proofs of the existence of a special system of trophic nerves and trophic nerve centres, distinct from those which minister to the general functions of motion (secretion) and sensation, or whether the maintenance of the nutrition of the tissues is merely a different aspect of the general functional activity of sensory and motor nerves.

He has, on the principle "entia non sub multiplicanda," been more inclined to adopt the latter view, and to regard, with Sigmund Mayer, and Dr. Seguin, the muscles, nerves, and anterior cornua as forming together a functional and nutritive unity. In combination these structures exhibit what we call normal laws of reaction and nutrition; but when separated they are reduced, each to its own independent vitality, and exhibit the changes which we are accustomed to call those of degeneration, characterized by the retrogression of the more highly organized, and proliferation of the less highly organized structures.

The recent important researches of Gaskell have thrown new light on the structure and functions of the nervous system, more especially the nerves which innervate the walls of the bloodvessels and the hollow viscera.

There are two sets of nerves possessing diametrically opposite functions. One set, *katabolic*, excite activity and cause waste; the other set, *anabolic*, inhibit activity and promote repair. It may be that the inhibitory or anabolic influence is not, like the katabolic or motor, the direct result of nerve stimulation but only a question of interference of nerves alike in function. But if the facts cannot be so explained, then there would appear to be (in the case of the muscles of organic life at least) a system of nerves which would be properly called trophic.

Whether this differentiation of nerves obtains also among the muscles of animal life is a point which has not been, as yet, determined.

He is of the opinion that more light will be thrown on this subject by the researches on thermal or heat centres now being actively pursued, and on which Dr. H. C. Wood and Dr. Ott have given us valuable contributions.

The question is whether there are nerve centres which influence and regulate the production of animal heat, apart from those which are related to motion and sensation. Many facts have been adduced in favor of this proposition, but, he thinks, much will have to be done before we can regard it as proved.

The muscles are the chief foci of heat-production in the body. He should, therefore, argue that the centres of heat-production must be coextensive with those which minister to the nutrition of the muscles. He is, therefore, not readily inclined to believe that there can exist, anywhere in the nervous system, any one centre capable of maintaining (as a nerve centre must) afferent and efferent relations with the whole muscular system. So far as yet appears from experimental research, increased production of heat has been found in association with irritative lesions of nerve centres and tracts; but he is not aware that it has been proved satifactorily that increased heat-production and increased tissue-waste result from the removal of a centre whose functions could, with propriety, be termed inhibitory or anabolic. But he thinks that researches in this direction are those which are best calculated to throw light on the difficult problem at present under discussion.

MR. VICTOR HORSLEY, of London, begged to be allowed to communicate the results of experiments performed by Dr. Frederick Mott, of London, in the laboratory of the Brown Institution, since it seemed to him that Dr. Mott's observations were of the utmost importance, especially in view of the direction which the present discussion had taken.

The question appeared to him to present the following points. Destruction of what we regard as trophic centres or cutting of fibres leading from the same may presumably produce disturbances of nutrition in one of three ways or combination of these, viz.:

(1) True trophic disturbance.
(2) Loss of function.
(3) Vasomotor changes.

Vasomotor changes, he thought, were anyhow put out of the question by the fact that experimental removal of the superior cervical ganglion of the sympathetic produces practically no change. Dr. Mott's method of experimentation appeared to him to possess the especial merit of excluding not only this complication, but also that due to loss of function. His method was as follows: He ligatured, on one side, certain anterior roots in the cauda equina of monkeys, under strict antiseptic precautions, and then observed the condition of the femora on the two sides. Now the normal metabolic changes in bone are, of course, known since the days of Hunter to be a combination of absorption and restoration. Dr. Mott found that the trophic changes in the bones produced were destructive—*i. e.*, exaggeration of the normal process of absorption. The femur of the side on which the roots were ligatured was probably atrophied, and on microscopic investigation Dr. Mott discovered that the medullary surface of the interior of the bone was thickly covered with osteoclasts. Considering that the function of the bony skeleton is so essentially passive, especially in contradistinction to the example of muscle quoted by previous speakers, and considering the small influence which vasomotor changes could be imagined to bear on the nutrition of such a resistent tissue as bone, he thought that Dr. Mott's experiments definitely supplied proof for the first time of katabolic changes originated by lesion of efferent nerve fibres.

DR. SEGUIN, in closing, said that he had been very much pleased to find that his conclusion and subdivision of these lesions have not met with more contradiction than they had received. He claimed that true trophic lesions are the result of some mysterious connection between the nervous system and organs in other parts of the body. With respect to the very interesting experiments of Dr. Mott, he would say that there is nothing new in the essence of the experiments. He was very pleased to hear no actual refutation of his conclusions.

DR. FRANCIS T. MILES, of Baltimore, read a paper on

THE EFFECT OF CONCUSSION OF THE SPINE ON THE REFLEXES.

He called attention to some lesions of the spine of a traumatic origin which, while cutting off the influence of

the will destroyed the reflex actions. This is a subject, he said, very little brought to the attention of the profession. Dr. Hammond, in his work on the *Diseases of the Nervous System*, gives a case in which the reflexes were lost. In all the series of cases collected in the *Medical History of the War of the Rebellion* nothing is said of the loss of the reflexes from concussion.

DR. R. T. EDES, of Washington, said that several cases of this kind came under his observation in the hospital in which, a number of weeks after the shock, there was very much exaggerated tendon reflex, a fact of much value in the present discussion. In one case, a male, he found him paralyzed and in a state of complete contracture. He had fallen on his back under the wheel of his wagon. On autopsy, the parts above the medulla oblongata were lost, but all down the cord there was that condition of sclerosis which is found associated with a state of exaggerated reflex.

DR. WM. OSLER thought that as a result of concussion there may be an abolition of the reflexes which may persist for some length of time. Then, in addition to that, there may be a complete abolition of the reflexes from a central myelitis and a destruction of the integrity of the gray matter. He had a case of that kind recently in Philadelphia. The cases to which Dr. Edes referred they had all seen.

DR. H. C. WOOD had seen a number of cases in which the traumatism ended in a subacute myelitis involving the whole cord. Then again, he had seen cases in which the traumatism ended in chronic lateral inflammation. For instance, a woman gets hurt, may be badly frightened into hysteria, the cord gets badly shaken, a minute hemorrhage perhaps occurs through it, and she gets a myelitis. We may have various lesions following traumatism, as from a blow on the head.

Dr. M. ALLEN STARR said that it seemed to him that in those cases of concussion in which the reflexes are arrested and in which there is no injury post-mortem to the part of the cord that governs the reflexes, there is the explanation on the theory of inhibitory impulses coming from a higher region and transmitted downward which arrest the reflexes. Such a condition exists in hemiplegia. No doubt in many of the cases of Dr. Mills the condition was one of irritation at a higher level and so arresting the reflex. He agreed with Dr. Edes that, if the patients lived long enough, the irritation would have ceased and they would have recovered with exaggerated reflexes.

AFTERNOON SESSION.

DEMONSTRATIONS IN PATHOLOGICAL ANATOMY.

DR. WM. H. WELCH, of Baltimore, demonstrated a series of

MICROSCOPICAL SPECIMENS OF THE THYROID GLAND AFTER PARTIAL EXTIRPATION.

It is a well-known fact, he said, that the extirpation of both lobes of the thyroid gland in a dog is followed by characteristic symptoms and the death of the animal. The longest duration of life after the extirpation of the gland was nineteen days. The symptoms made their appearance usually within two or three days after the operation. The first symptoms were fibrillary twitchings of the muscles of the tongue. Similar tremors made their appearance in the other muscles of the body. Tetanic spasms of the muscles of the extremities were very commonly present. Trismus, difficulty in respiration, sometimes labored, and sometimes an alteration of the rhythm of respiration. Sometimes paroxysms of dyspnea. The animal very soon became apathetic, lay down most of the time, could only be roused by noise, but very readily by touching him. In many cases there was observed pallor of the gums of a scorbutic character. Ulceration of the cornea and inflammation of the conjunctiva were frequently present. The hair fell out, the skin itched, and a thickening of the skin was quite noticeable about the cheeks and forehead. A tendency to walk backward and difficult deglutition had been observed. The extirpation of both lobes of the thyroid gland is, therefore, followed by a characteristic group of symptoms and death of the animal.

Last year Prof. Münck, of Berlin, isolated the two lobes of the thyroid gland in a dog by tying off all their vascular and nervous connections, left them *in situ*, and sewed up the wound. The experiments of Münck induced Dr. Halstead to undertake another series of experiments.

A word as regards the normal anatomy of the thyroid gland. After isolation of a single lobe of the thyroid gland according to Münck's method, interesting changes are observed in the remaining lobe. The first noticed is an enlargement of the epithelium. This is accompanied with dilatation of the capillaries. For forty, fifty, or eighty days the change goes on until finally the structure of the gland is so altered as to be no longer recognized as the former thyroid gland.

DR. T. MITCHELL PRUDDEN, of New York, then demonstrated some

SECTIONS OF ATROPHIED THYROID GLAND FROM A CASE OF MYXŒDEMA,

described by Dr. Henry Hun in *The American Journal of the Medical Sciences* for July, 1888. Attention was especially called to the large amount of lymphatic tissue which was present in and near the small remnants of glandular structure of the thyroid.

DR. A. JACOBI then read a paper on the

PATHOLOGY OF THE THYMUS GLAND.

It has been, he said, the alleged hypertrophy of the thymus gland that has atrracted most attention. It is not a very uncommon thing to find a thymus gland weighing twenty-three grammes. He has seen a few cases of sudden death in an infant and absolutely no assignable cause of death except a large thymus gland. If we find a thymus gland of the thickness of more than one centimetre, it is quite natural that the slightest change in the circulation of such a gland should so interfere with the neighboring part as to bring on sudden death. Medullary sarcoma has been first described by Astley Cooper. There is a case of hemorrhagic sarcoma, undoubtedly a sarcoma of the thymus gland with round cells. Virchow mentions a case of sarcoma of the thymus and supposes it can reach the pleura aud pericardium in such cases.

Now, it being a fact that the literature of the thymus is very scanty and there being very few observations of disease of the thymus in our literature, it occurred to him thirty years ago that it would be worth while to look

after the thymus in every post-mortem and notice what has occurred. He had, therefore, gathered a certain number of cases in which the thymus was affected, either primarily or secondarily, together with other diseases.

A number of thymus glands have been examined and he had some positive results in regard to tuberculosis, syphilis, diphtheria, etc. Evidently the thymus is an organ in more constant change than any other organ in the human body. He has met with cases in which the thymus was reduced to a very small size by being pressed upon by a tubercular tumor of the neighboring parts. In infants who are tubercular, tuberculosis of the thymus is not very rare. It is found in small tubercular deposits of a miliary kind. There is no doubt that syphilis in the thymus gland is found in two forms as in other organs. It is found as an increase of connective tissue as in liver, spleen, and kidneys in other diseases. It is further found as gummata. There is nothing very characteristic about the changes of diphtheria. They are the least conclusive of all.

DR. GEORGE M. STERNBERG then read a paper on his

RECENT RESEARCHES RELATING TO THE ETIOLOGY OF YELLOW FEVER.

He said he would make a demonstration of the methods of research and give in a general way the result of his investigation up to the present time. As to the specific etiological agent, he had not arrived at any definite result, yet he has done a good deal to make the way clear for other observers, and hoped they may yet arrive at definite conclusions in this respect. To make an analysis of the bacteria found in the intestine of a single individual would require at least six months. He had made ten autopsies in Havana. His general plan of procedure was as follows: As soon as notified that he could make an autopsy, he went to the hospital and made it as promptly as possible. He took blood from the heart, material from the liver, spleen, kidney, and the bladder through its walls, and the stomach and intestines. From the kidney and liver he got organisms in a certain proportion of cases. In six he had a negative result from the liver and four from the kidney. In the majority of cases, from the urine drawn from the bladder he had a negative result. In two out of ten cases he got organisms. In no case did he get the organism which Freire has described, either from the blood, tissues, or alimentary canal. In five cases in which he took considerable pieces of liver and kidney and immersed them in a one per cent. solution of bichloride of mercury, wrapped them up afterward in a piece of muslin saturated with bichloride of mercury several thicknesses, put these pieces away in a laboratory for forty-eight hours, then taking off the wrapping and cutting into the organ, in every case he obtained organisms. In no case did he get the micrococcus of Freire.

In his cultures he found an organism which was altogether different from any heretofore described by reason of the mode of its growth in gelatine. In the first case from the intestine and stomach he got numerous colonies of an organism which, put in twenty per cent. gelatine, gave him colonies of a peculiar form, and he supposed he had a microörganism. In the next two cases he got different colonies.

In Executive Session the following were elected

OFFICERS FOR THE ENSUING YEAR:

President.—Dr. Francis Minot, of Boston.
Vice-Presidents.—Drs. R. Palmer Howard, of Montreal, and S. C. Busey, of Washington.
Recorder.—Dr. I. Minis Hays, of Philadelphia.
Secretary.—Dr. Henry Hun, of Albany.
Treasurer.—Dr. W. W. Johnston, of Washington.
Councillor.—Dr. George L. Peabody, of New York.
The following were

ELECTED TO MEMBERSHIP:

Drs. Morris J. Lewis, of Philadelphia; D. W. Prentiss, of Washington; Norman Bridge and J. N. Danforth, of Chicago; Charles L. Dana, of New York; and Harold C. Ernst, of Boston.
Adjourned.

NEW YORK ACADEMY OF MEDICINE.

SECTION IN NEUROLOGY.

W. R. BIRDSALL, M.D., CHAIRMAN.

Stated Meeting, October 12, 1888.

DR. CHRISTIAN A. HERTER read a paper on

THE THERAPEUTIC USES OF HYPNOTISM.

His object, he said, was to direct attention to the important therapeutic uses of this agent and to the best methods of employing it. The employment of hypnotism is attended with difficulty, on account of the prejudices existing against it in the minds of the profession, as well as the public at large. It is, indeed, a subject that is still *sub judice*, and hence it is necessary to quality any conclusions that may at present be arrived at concerning it. Hypnosis resembles ordinary sleep, with the element of "suggestion" from the operator added. Suggestion is effective, however, in the waking state also, as shown in the so-called Christian science and faith-cure, as well as to some extent as regards homœopathy.

Dr. Herter spoke at some length of the researches of Lieboldt, and described the six degrees into which he had divided hypnotic trance. This was at variance with the well-known division of Charcot, into catalepsy, lethargy, and somnambulism. Charcot's cases, he thought, should not be taken as a basis of conclusions because they were, as a rule, trained subjects. He had never seen the neuro-muscular excitability, so much spoken of, occur spontaneously, although he had found that it could be readily secured by suggestion.

He then described the proper method of producing hypnosis, as advocated by Lieboldt and Bernheim. The operator should first secure the patient's confidence, and then tell him to look him steadily in the eye and think of nothing but going to sleep. The process could be materially aided by suggestive remarks addressed to the patient, and by placing two fingers upon the face; the fingers being finally pressed gently upon the eyelids. In order to obtain good results it is necessary to watch the patient very closely, and to suit the words to the symptoms of sleep as they develop themselves. The *rationale* is very simple. Verbal suggestion in the

waking state makes the patient believe that he is falling asleep, and the method closely resemble the natural process. It had been found that men and women are about equally susceptible to hypnotism, the preponderance being only one per cent. in favor of the latter.

Dr. Herter next spoke of some of the conditions in which it had been employed as a therapeutic agent. He first referred to hysteria, which, he said, had been more written about than any other in this connection. Notwithstanding the literature that has accumulated concerning it, however, the special indications for resorting to hypnotism in it are still far from clear. It is probable that hypnotic suggestion might be of more or less service in all forms of hysteria; but, as a rule, the recovery, if secured, is not permanent. In old cases of contractures due to this cause, it is indeed often very difficult to get any appreciable result. The effect on hysterical amblyopia and amaurosis is extremely interesting, and in many cases a very marked improvement in vision has attended the persevering use of hypnotism. Of the motor paralyses, none is more benefited by hypnotism than abductor paralysis of the larynx. In hysterical convulsions the number and severity of the attacks are greatly diminished. He said that one need never be discouraged by failure to secure hypnosis at the first trial in any case of hysteria. The prospect of a cure would depend on various circumstances, and in many instances hypnotism would at all events serve as a useful adjunct to other treatment. Not much is to be hoped from it in hystero-epilepsy. He had personally employed it in two cases, but with no satisfactory results. In one of the cases profound stupor was induced, which resembled post-epileptic coma. The prospect was not encouraging in epilepsy either.

In several cases of chorea the effect on the movements was rapid and marked, and with perseverance a cure might sometimes be made in this affection. A number of daily sittings are generally required, and in some cases it is necessary to keep these up for months. Still, in any case of supposed cure there would always arise the question whether the disease had not simply run its course and ended spontaneously. There was the most ground for hope in those cases in which the movements were general, and the previous existence of rheumatism and endocarditis did not seem to render the chances of relief less favorable. In insanity the results from hypnotic suggestion have not, for the most part, been satisfactory. He had seen it tried quite extensively in the insane asylum at Zurich, Switzerland, but had never observed any benefit that was not evanescent. The largest number of experiments had been made in cases of paranoia, but the dissipation of the hallucinations sometimes effected was only temporary.

In delirium tremens its effect is often excellent, and quiet, restful sleep was sometimes secured by it in cases in which the same result could only have been obtained by the use of drugs in doses so large as to be dangerous. Alcoholism constitutes one of the most hopeful fields for its employment. The *séances* should be given twice daily for several months, and in many cases the patient becomes inspired by suggestion with disgust for drink. The same is true also of the opium, chloral, cocaine, and tobacco habits; but in all these cases surveillance in an asylum is, of course, an essential feature of the treatment.

Masturbation and other bad habits had been cured by hypnotism, and perhaps no more satisfactory results from its use had been observed in any other affection than incontinence of urine in children. Seiboldt had employed it in seventy-seven cases with a very large percentage of cures in those which remained under treatment for any length of time. The average age of the patients was seven years, and in two of these cases there was incontinence of feces in addition to the enuresis. The result obtained was the more gratifying from the fact that many of the cases had resisted all the ordinary methods of treatment. In various rheumatic affections Bernheim and others had reported fairly satisfactory results. Among these were lumbago and subacute, chronic, and gonorrhœal rheumatism. In chronic joint cases it was claimed that not only was the pain diminished, but that the nutrition of the joint was actually improved by the treatment. Dr. Herter thought that it was doubtful, however, whether any real effect upon the joint was produced. In neuralgias much benefit could be affected in recent cases, but if the trouble was chronic there was less chance of success. In some old cases of sciatica, however, good results had been claimed, and, at all events, the method was often of service in securing sleep, even if no permanent benefit could be looked for. In functional headaches it was of decided service, and in certain instances of migraine he had been successful in mitigating and cutting short the attack by its aid.

At first sight, it might seem that such an agency as this could have no effect upon a function like that of menstruation; but it had been practically demonstrated that in some cases not only the interval between the menstrual periods but the length of the periods themselves could be absolutely controlled by it. The number of individuals in which such results as these could be obtained was, however, comparatively rare.

As to the employment of hypnotism in surgery as a substitute for the ordinary anæsthetics the number of patients, with the exception, perhaps, of children, in whom it was available for major operations, is, no doubt, small, especially as the anticipation of having an operation performed is extremely apt to interfere with the success of hypnosis. On the whole, its employment in this field is not to be recommended, he thought, except in cases in which, for any reason, the use of anæsthetics was contraindicated. He had, however, seen a molar removed under its influence without the patient suffering any pain, or having any remembrance afterward of the extraction of the tooth.

Having spoken at some length of the use of hypnotism in parturition, Dr. Herter said that we are justified in arriving at the following conclusions regarding this subject:

(1) It induces sleep, and it is in no wise prejudicial to the efficiency of the uterine contractions.

(2) It has no tendency to produce post-partum hemorrhage, and is unattended by any other bad results.

(3) It is in no way comparable to chloroform in labor and should only be used in those rare cases in which the usual anæsthetics are contraindicated.

In insomnia it can often be employed with good results, but it sometimes requires months of persevering treatment to effect a cure. In cases in which drugs have been resorted to it can be used in addition to the habitual remedy, the dose of the latter being gradually

diminished, and perhaps finally exchanged for a placebo. Among the other affections in which it has been tried are writer's cramp and paralysis agitans.

Among the objections which have been raised to the employment of hypnotism are the following:

(1) That it tends, in certain susceptible subjects, to produce temporary mental alienation; some individuals thus developing hallucinations during wakefulness.

(2) That some individuals fall asleep spontaneously, showing a tendency to auto-hypnotism. (This tendency, however, can always be antagonized by suggestion.)

(3) That some can readily be put to sleep by any one who chooses to do so. (This can also be antagonized by suggestion.)

In conclusion, Dr. Herter suggested the following general rules for those practising suggestive therapeutics:

(1) Never hypnotize any subject without first obtaining his or her formal consent.

(2) Always hypnotize in the presence of a third person.

(3) Never give any suggestions other than those which are necessary for the patient's improvement in health.

Public exhibitions of hypnotism, he thought, should be prohibited by law, and the use of this agency exclusively confined to the medical profession. One advantage connected with its employment in medicine is that it does not lose its effect by repetition. On the contrary, the subject becomes more and more readily influenced by it the oftener he tries it. From what had been said it could be seen, therefore, that suggestive therapeutics is not a delusion; and he had no doubt that, before many years, it will take a regular place in medical science. (At the conclusion of his paper, Dr. Herter exhibited on a female subject the method of inducing hypnosis.)

DR. BIRDSALL then read the

REPORT OF A CASE OF TREMOR TREATED BY HYPNOTIC SUGGESTION.

The patient was a young German, male, twenty-one years of age, who came to his service at the department for nervous diseases at the Manhattan Eye and Ear Hospital, suffering from tremor of the right forearm, which had made its appearance a short time previously, and which completely incapacitated him for his ordinary work. There were rapid flexion and extension of the arm, and the movements were so violent as to shake his entire body. A thorough examination failed to detect any organic disease of the nervous system or any other part of the body. His general health was unimpaired and his habits appeared to be exceptionally good. As he seemed to be rather an impressionable subject, it was determined to try the effect of hypnotism upon him. Hypnosis was readily induced, the arm becoming perfectly quiet under its influence, and he was told that when he awoke the tremor would not return. When he first came out of the trance there was no motion, but it soon came back to a slight extent. He was told, however, that it would stop altogether before long, and requested to return for a second treatment the following day. The treatment was repeated several times, and after the fifth *séance* the cure was complete. Later, when Dr. Birdsall had gone out of town for the summer, the patient returned to the hospital stating that the trouble had returned to a slight extent. It was again entirely cured by the same treatment, administered by the physician in charge, and some time after that the patient left town to reside in the South; since which he had been lost sight of.

This was, no doubt, a hysterical case, and to detect whether the young man was shamming or not the most severe tests were applied, both in reference to the tremor and the genuineness of the hypnosis. Dr. Birdsall said, in concluding his report of the case, that he believed that we are not justified in wholly discarding a method of treatment which has already shown itself so useful; though, of course he did not mean to contend that we should by any means attempt to treat all classes of cases by hypnotism, and thus place ourselves on a level with the faith-healers and Christian science people.

DR. OSGOOD MASON read a paper on

SIX CASES OF TREATMENT WITH ANIMAL MAGNETISM.

He said, in commencing, that as the term hypnotism is not altogether satisfactory, and as he had been in the habit of employing that of animal magnetism before the latter came into vogue, he still preferred to retain it, in the absence of any better designation. The first of his cases he had treated eighteen years ago, and was that of a female completely broken down in health from overwork and anxiety, who suffered especially from headache. The treatment by hypnotism was repeated ten or twelve times with great improvement to her health and comfort. The second case was that of a female, twenty-two years of age, suffering from hysterical paralysis and subject to hallucinations and violent screaming fits, and she also was greatly relieved. The third patient was an Irish girl of fifteen, who suffered from epileptic attacks. The bromides were first tried without any appreciable effect. Under treatment by hypnotism, however, there was marked improvement, although she did not remain very long under observation. The fourth patient was a lady of gouty diathesis, who suffered from chronic rheumatism and was in wretched health generally. She was treated twice a week for three months, at the end of which time she returned to England, where she had formerly lived, greatly improved. The fifth patient was a lady who suffered from a painful neuritis of a traumatic origin, and finally died from inanition. Her case was referred to simply on account of one peculiar effect produced by the hypnotic treatment. It was a remarkable fact that every time that the treatment was repeated her hand, which was usually cold and of a blue color, was temporarily restored to its normal condition. The sixth patient was a girl of fifteen, suffering from epileptic attacks, who came under observation August 31, 1885. Finding that medicinal treatment was entirely unsatisfactory, he tried hypnotism with very gratifying results. The *séances* were repeated twice a week, and the number of the attacks, which at first occurred from six to ten times a day, was soon reduced to one or two a week, and afterward to one or two a month. In about five months they disappeared altogether, when the treatment was discontinued. Three weeks after this was done, however, she had two attacks; in consequence of which the treatment was resumed, and kept up regularly for three months. Since then there had been no return of the trouble whatever.

The deductions which Dr. Mason said he had derived from these and other cases which he had observed were as follows:

(1) Animal magnetism is of decided value as a therapeutic agent.

(2) Sleep is not always necessary for the production of the greatest benefit.

(3) It has the effect of equalizing the circulation.

(4) It also regulates and equalizes nervous action.

Corollary: The equalization of the circulation is the result of the equalization of nervous action.

Mr. Corey, of Boston, Chairman of the Committee on Hypnotism of the American Society for Psychical Research, then read a paper on

THE THERAPEUTIC VALUE OF HYPNOTISM.

He spoke particularly of the results reported by M. Voisin in certain cases of insanity. He also referred to many of the affections spoken of in Dr. Herter's paper, and expressed the opinion that hypnotism would undoubtedly prove of great service in properly selected cases in medical practice. In the latter part of it he dwelt at some length upon the medico-legal aspect of the subject, and spoke of the danger that hypnotized individuals might be induced by the unscrupulous to commit criminal acts.

Dr. Kremer said that his experience with the method had been comparatively small. He had, however, tried it in six or eight cases of a neurasthenic or hysterical character. Three of these presented points of special interest, and he said he would relate them. The first was that of a young girl with hysterical aphonia, who readily succumbed to the hypnotic influence. He was, however, unable to make her speak while hypnotized, and three or four months afterward the aphonia got well spontaneously. It seemed to him that the effect of hypnotism on this patient was bad. It was a fact worthy of note that, on one occasion, he extirpated an ingrown toenail for her, while hypnotized, without her feeling any pain. The second case was one of paralysis with spastic contraction, so that the patient could not put the heel to the ground. Under the influence of hypnotism the spastic condition entirely disappeared, and the knee-jerk, which had been abnormally marked, diminished. Otherwise she might have been treated for lateral sclerosis. The third case was one of hysterical cough, in which there seemed to be a hyperæsthetic condition of the entire respiratory tract. Cocaine was employed for a considerable time, but noticing a tendency to cocaine poisoning, Dr. Kremer employed hypnotism. The cough was completely stopped, but the alleviation was only temporary. He thought that in this class of cases there were other remedies which would act as well as hypnotism, and that in cases of neurasthenia it had a general depressing effect. In one case he had had considerable difficulty in arousing the patient from the hypnotic sleep, and in several instances he had found that the treatment gave rise to headache and malaise. On the whole, his experience with the method had not been very favorable.

Dr. C. L. Dana mentioned the case of a child of a neurotic family, who was suffering from vomiting and intense headache, in which, after other remedies had proved inefficient, complete recovery was secured by hypnosis repeated twice. After referring to two other cases, he said that it was something of a coincidence that at the time when the convention of the "Christian Alliance," composed of those who believed in faith-healing, was treating the subject of suggestive therapeutics empirically, it should be discussed at the Academy of Medicine from a rational point of view. He said that five or six years ago, when the method of Bray was in vogue, he had studied hypnotism with considerable care, and he had known of one subject, a young girl of nervous temperament, who was affected with convulsive attacks in consequence of the treatment. While the methods of Bernheim and Lieboldt are much less objectionable, he did not think that the practical application of hypnotism was of very wide range. There are comparatively few individuals who are suitable subjects for treatment by it, and there are but few physicians who are willing to devote the time necessarily required by the method.

Dr. Dana thought the so-called faith-cure and Christian science should be prohibited by law, and expressed the opinion that it was better for a person to remain ill rather than to be cured by such agencies, which tend to produce paresis of the will and eventually to bring about a certain amount of volitional degradation.

Dr. Herter said, in reply to Dr. Kremer, that he had never had any difficulty in arousing a patient from the hypnotic sleep, although he had seen the method tried on more than one hundred subjects. The only exception was in an individual who had been in a spontaneous condition of trance previously. He did not think this a valid objection to hypnotism, and as to the headache and malaise referred to, he thought these could always be prevented by suggestion before allowing the patient to awaken. He had never noticed any depressing tendency about the treatment. He agreed with Dr. Dana entirely as to the bad effects of Bray's method with the bright light, which had been known to give rise to epilepsy and convulsions, but said that he did not know of any evil results from the method now employed. It is comparatively easy to hypnotize a considerable portion of people, although he thought that in the higher classes of society it is more difficult to get subjects sufficiently passive than it is in people of the middle and lower classes.

NEWS ITEMS.

The Philadelphia Polyclinic Course of Evening Lectures.—The second annual series (Part I.) of evening lectures on practical subjects will be given by the Faculty of the Philadelphia Polyclinic, at the college building, Broad and Lombard Streets, free to members of the profession and to medical students, and in the following order, beginning at 8 o'clock:

October 23d, Prof. Henry Leffmann, "The Recognition of Poisoning."

October 30th and November 6th, Prof. Thomas J. Mays, "Pulmonary Consumption Considered as a Neurosis."

November 13th, Prof. Charles K. Mills, "Visual Localization."

November 20th, Prof. J. Henry C. Simes, "Pseudo-sexual Disorders."

November 27th, Prof. S. Solis Cohen, "The Management of Fever, with especial reference to High Temperatures."

December 4th, Prof. H. Augustus Wilson, "The Preparation and Application of the Plaster-of-Paris Bandage."

December 11th, Prof. John B. Roberts, "Treatment of Leg Ulcers."

December 18th, Prof. Edward Jackson, "Failure of Sight in Old People."

January 8th, Prof. Charles H. Burnett, "Some Common Causes of Deafness; Prevention and Cure of the Disease."

January 15th, Prof. Alexander W. Mac Coy, "Observations on Certain Forms of Hoarseness."

January 22d and 29th, Prof. Benjamin F. Baer, "Pelvic Inflammation: its Causes, Complications, and Treatment."

Part II. will be announced hereafter.

Disinfection in Cases of Contagious Disease.—In a disinfecting establishment in Berlin, one may have disinfected by compressed steam any article capable of absorbing infectious matter. Two marks (fifty cents) are charged for calling for and delivering, including disinfection, of articles occupying the space of one-half cubic metre (0.65 cubic yard); and four marks (ten cents) for each additional one-tenth cubic metre (0.15 cubic yard). The articles are returned within forty-eight hours. From April 1 to December 31, 1887, "in only a small proportion of the cases of contagious diseases officially recorded was this means of disinfection used, and in many cases probably all articles which might be infected were not sent." During this time, some ten thousand cases of infectious disease were reported, and disinfection was employed in seventy-four thousand. The best results were seen in diphtheria (15.9 per cent.).

The police department of Berlin requires unequivocal disinfection in case of Asiatic cholera, of variola, of typhus and relapsing fevers, and in diphtheria, and disinfection when officially directed in case of scarlet fever, of epidemic dysentery, of measles, of whooping-cough, and of phthisis. Besides, disinfection of dwellings is obligatory in the affections mentioned, consisting of scrubbing with five per cent. solution of carbolic acid, and rubbing the walls with bread. Guttmann recommends spraying, with a suitable apparatus, the walls with a one per cent. solution of corrosive sublimate, and, subsequently, wetting the walls with a one per cent. solution of carbonate of sodium, to render the sublimate innocuous. The oxychloride of mercury is precipitated, which may be readily removed. Dr. Hanstein states that a one per cent. solution of corrosive sublimate may be unhesitatingly applied, alone or with a five per cent. solution of carbolic acid. Those engaged in the work have not suffered in the slightest, if they have worn masks. The disinfection of a moderately large room occupied two workmen three hours.—*Centralblatt für die gesammte Therapie*, Sept. 1888, from *Berliner klin. Wochenschr.* No. 22, 1888.

Bacteriological Investigations into the Water and Air of the City of Warsaw.—Following are the results of the investigations of BUJWID into the condition of water and air of the city of Warsaw. No pathogenic microörganisms are ordinarily found in the air; it therefore follows that disinfection of hospitals and operating rooms is of no practical value or significance. Certain microörganisms which may be found in the air Bujwid failed to find during investigations over a period of three years. Of those found in the air non-pathogenic, but one not yet known gives rise to suppuration in mice and rabbits; the rest are innocuous. The number of bacteria in the air is subject to great variations and depends largely upon the winds and conditions of habitation. After a rain or snow the number is smaller. Basement rooms and abodes contain the largest numbers. On an average, one hundred thousand times more bacteria are found in water than in air. Good water should not contain more than three hundred rod bacteria to the cubic centimetre (0.06 cubic inch). Different results were obtained from the examinations of water from various springs and running streams; above the city the water contained about three hundred bacteria, in the midst of the town over fifty thousand to the cubic centimetre. Bujwid found no pathogenic microörganisms whatever. After filtering this same water (sand filters have recently been introduced into Warsaw) the proportion of bacteria diminished from twenty to sixty. Spring water contains a still larger number of microörganisms. In the discussion which followed, Barzyci stated that in a village near the city of Rzeszow, having no spring, a peasant living by the creek was affected with typhoid. His linen was washed in the stream. Shortly, many of the inhabitants, who obtained their drinking water from the creek and who lived below the house in question, likewise sickened with typhoid; all living above escaped.—*Wiener medizin. Presse*, No. 34, 1888.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF OFFICERS SERVING IN THE MEDICAL DEPARTMENT, U. S. ARMY, FROM OCTOBER 8 TO OCTOBER 15, 1888.

MEACHAM, FRANK, *Major and Surgeon* (U. S. Army).—Died at Fort Douglass, Utah Territory, October 9, 1888, at twenty minutes past 1 P. M.

By direction of the Acting Secretary of War, LOUIS M. MAUS, *Captain and Assistant Surgeon*, is relieved from duty at Fort Schuyler, New York, and will report in person to the commanding officer Fort Ponter, New York, for duty at that post.—Par. 10, *S. O. 236, A. G. O., Washington*, October 10, 1888.

By direction of the Acting Secretary of War, leave of absence for two months is granted WILLIAM G. SPENCER, *Captain and Assistant Surgeon* (U. S. Army).—Par. 11, *S. O. 236, A. G. O., Washington*, October 19, 1888.

OFFICIAL LIST OF CHANGES IN THE STATIONS AND DUTIES OF THE MEDICAL CORPS OF THE U. S. NAVY, FOR THE WEEK ENDING OCTOBER 13, 1888.

BATES, N. L., *Medical Inspector.*—Detached from the "Pensacola," and placed on waiting orders.

FARWELL, W. G., *Surgeon.*—Ordered to U. S. Receiving Ship "Franklin."

SCOTT, H. B., *Passed Assistant Surgeon.*—Detached from the New York Navy Yard, and ordered to the Naval Hospital at Mare Island, California.

MAGRUDER, A. F., *Surgeon.*—Ordered to duty at headquarters of Marine Corps.

THE MEDICAL NEWS *will be pleased to receive early intelligence of local events of general medical interest, or of matters which it is desirable to bring to the notice of the profession.*

Local papers containing reports or news items should be marked.

Letters, whether written for publication or private information, must be authenticated by the names and addresses of their writers—of course not necessarily for publication.

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